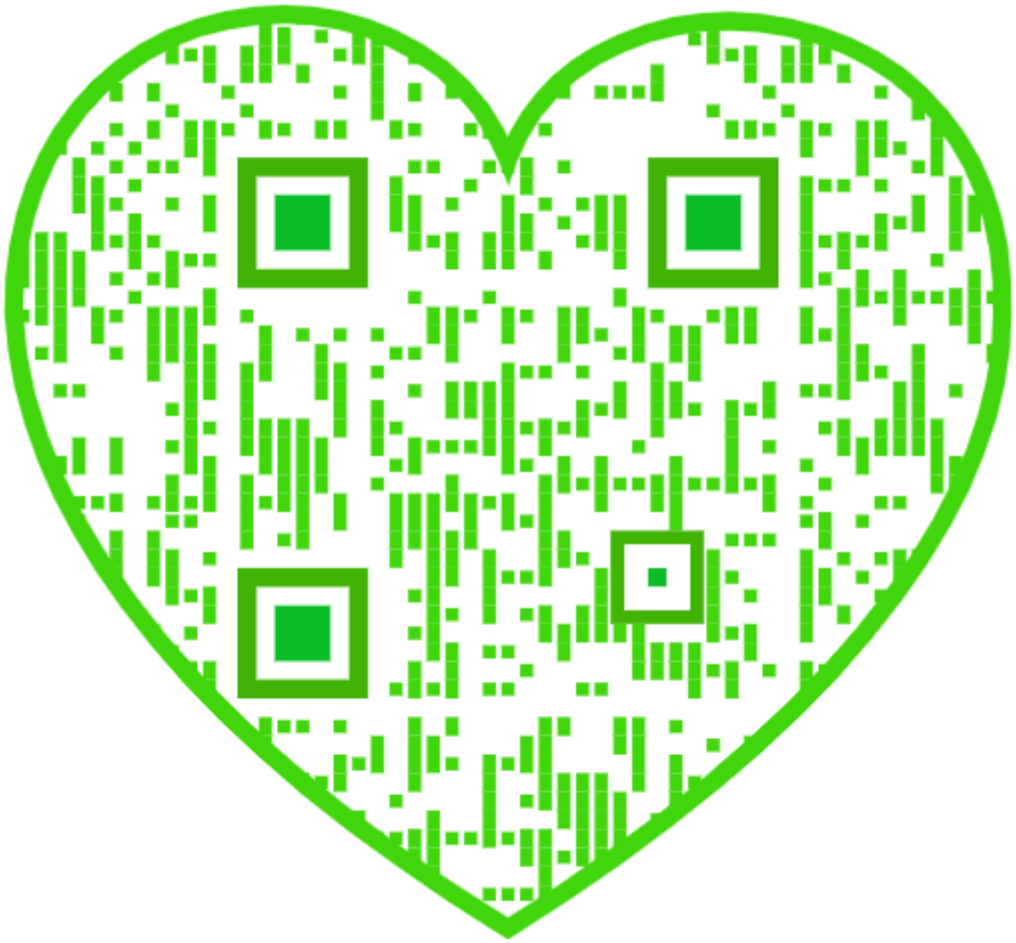

A CRITIQUE OF ECONOMICS

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A CRITIQUE OF ECONOMICS

DOCTRINAL AND METHODOLOGICAL

BY

O. FRED BOUCKE

Professor of Economics at Pennsylvania State College

AUTHOR OF "THE LIMITS OF SOCIALISM" AND "THE DEVELOPMENT OF ECONOMICS, 1750-1900"

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PREFACE

On account of recent developments in philosophy and science we are today again confronted with the important question as to whether economics is really a science or only a study of values akin to philosophy proper. If economics is a science comparable to physics or chemistry, for example, applications in both private and public life may be possible; if not, the practical value of economic research must be slight, whatever our interest in it on other grounds.

This book is an attempt mainly to show in what sense economics may be called a science, and what changes seem necessary to bring it into harmony with current facts and concepts in allied fields. The first part is entirely critical, following other writers who have broken with either Utilitarian (classical and neo-classical) or Marginal economics, or with both. It deals with the errors of the old psychology on which economic theorizing until recently was based, and supports the well-known contention that economic laws so far have been not so much laws as proofs derived from certain more or less arbitrary assumptions. To this extent then the first part goes over familiar ground and leads simply to a negation.

The second part, however, is meant to be more than a criticism. It seeks to make clear not only that new points in method must be stressed if old premises are abandoned,

but also that this question of method must be connected with current views in psychology, philosophy, logic, and ethics. That the rejection of eighteenth century sensationalism would prove fatal to many economic "laws" might be taken for granted. That for the same reason however the relation of induction to deduction, of statics to dynamics, of statistics to induction, or of economics to ethics should also be restated is not self-evident. Yet this is the belief of the present writer, and hence his endeavor to sketch in outline a new methodology of economics. The second part of the book thus is constructive as well as destructive, and has a bearing on all social inquiries, not merely on economics.

In saying this, however, the writer wishes to call attention to three points:

In the first place the logic of the principal argument in this book will be broken if its several chapters are not read in the exact order here given. Any departure may lead to misunderstandings with regard to a particular problem.

In the second place the approach here made is tentative, and not by any means categorical, in spite of a rather positive tone here or there. It is virtually impossible to preserve at all times the interrogatory form, however great our desire to admit the subjective nature of all inquiry, and especially of a critique of economics passing through a transition stage.

In the third place, the writer of these lines is keenly aware of his obligations to other writers, living and no longer alive. A bibliography has been appended in order to indicate partly the scope of this indebtedness. But it

will necessarily be very fragmentary. For the rest, therefore, the acknowledgment must be implied rather than expressed. It is the text which shows what is borrowed and what is new, and to this the expert will turn in making his appraisal.

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A CRITIQUE OF ECONOMICS

CHAPTER ONE

THE PROBLEM

How Economics Became an Exact Science.—The founders of economics as a science had very definite hopes regarding it.[1] They wished to show that laws prevailed in the realm of psychics no less than in that of physics. They differed from the Kameralists and Mercantilists in that they cared little about mere description of individual events or institutions, and much about the discovery of principles which should make applications in government reliable and fruitful. It was held by the Physiocrats and by Adam Smith that the Newtonian system could not stand alone in the cosmos, that mind and human actions in general must surely have their laws too, and that a continuity from molecule to man was part of the plan of the Creator.

The eighteenth century carried on the work of the preceding one of course, and there was a great variety of beliefs on things philosophical. Most of the viewpoints in metaphysics and epistemology, which have since then

[1] Discussed in the writer's "Development of Economics," of which the present book is, in a sense, a continuation.

come to definite expression, found currency in one form or another even at that time. It would be wrong to say that the age of Voltaire was preëminently materialistic, or phenomenalistic, or empirical or rationalistic, and so on. We cannot identify so broad a stretch of time, a particular system of speculation. But there is no doubt that the men who made out of Kameralism a science, or essayed to do so, leaned strongly toward a monism that coupled mind with matter, acknowledging but one Design in the workings of organic and inorganic existence, and believing implicitly in the possibility of proving social events as regular, as truly subject to law and causation, as the substances, the forces, that had been studied so successfully by Galileo and his disciples. The fathers of economics, of political economy, were more interested in disclosing laws of feeling, valuation, and action than in devising ways and means for filling the public coffers. The needs of the state were not slighted. Nobody overlooked them, or thought them unsuitable for study by theorists. The eighteenth century was so filled with wars and striking changes in the economic environment that public revenue and expenditure was sure to form a fascinating topic. But on the other hand the scientific spirit, the aim at a formulation of laws which should show what governs the present and the future—this thought animated most of the men who lifted economics out of its obscurity, developing it into the first of modern social sciences.

The Physiocrats were outspokenly materialistic and stressed the circulation of wealth as a counterpart to the circulation of blood in the human body, both correspond-

ing, as it seemed, to the orbits of the planets in the cosmos. This naïve, physical view of socio-economic happenings had its disadvantages, but it meant from the start a concern for the thing-aspects of wealth, for the physical volume of production, for progress measured by output and income in tangible items of consumption; and this was a wholesome interest. To the Scotch philosopher who published his "Wealth of Nations" in 1776 the Physiocratic attitude was not altogether intelligible, or at any rate acceptable. He agreed with Quesnay on many points, but put the importance of labor far above that of the fertility of the soil, and furthermore took a psychological view of the economic process that has dominated us ever since. It would not be worth while to rehearse the circumstances of the rise of the science of economics, except for this faith in laws, equal to those of the Newtonian, and for the firm belief of Naturalistic economists since Adam Smith, that human nature is the basis of such laws. The fact that the author of the "Wealth of Nations" had much earlier written a "Theory of the Moral Sentiments," while other British thinkers had turned out a long series of psychological studies both from a moral and a logical standpoint, this fact should never be forgotten in our appraisal of economics to-day, especially when we contemplate its present perplexities.

Smith first emphasized the balance of forces that a wise Providence displayed everywhere in the universe. He was profoundly impressed with a play of forces that meant peace and prosperity in the long run. He opposed sympathy and altruism to self-interest, and derived his cosmopolitanism and doctrine of *Laissez Faire* from this inherent goodness of human nature. Individualism meant

egotism, but also solidarity of aims. Human nature was the foundation of all wealth and its augmentation. What individuals thought and endeavored was the key to the principles manifested in price, income, and productivity. These fundamentals back of the pricing process took first place in the consideration of things human. The founders of economics insisted more upon the premises of their science than upon the principles of price or the circulation of wealth.

Those who are customarily called the English classics, to wit Malthus, Ricardo, Mill (father and son) and Senior, were just as objective in their analysis as the Naturalists, i.e., Physiocrats and Smith. The facts of economics were still *things* as much as values, and the non-pecuniary standard was conspicuous in the writings of the time. Physical productivity and costs as expenditure of time or of tangible goods were real to these Utilitarians—if we may call them so because of their hedonistic psychology and ethics.

But they differed from the Naturalists in substituting price and factorial shares for production and exchange. They gave a preëminence to the problem of price and distribution that has marked economics up to our own day. They brought in such concepts as labor-pain, talked of abstinence as an element in rates of interest, and gave to utility a higher rating as a determiner of price than their predecessors could have allowed. And above all, they admitted frankly their hedonistic outlook. What Bentham and others before him had said in explanation of our moral codes and of our conduct in private and public, the economic Utilitarians took up as an arcanum, a precious stone of wisdom, that might explain the opera-

tions of the economic system. Sensationalistic psychology colored economics from the turn of the century on, and has done so up to the present moment.

It was due to one man chiefly that this advance of economics as an "exact science" was so steady and rapid, and this man was John Stuart Mill. While others had made clear the bearing of legal premises upon the analysis of price and income, while much pains had been taken to show the laws of nature manifesting themselves in a "normal" price, or in the growth of the national income, it was left to the younger Mill to point out why and how economics could be a science comparable with physics. Precisely as Comte had proceeded to found a "social physics," calling it sociology, so J. S. Mill went ahead finding for economics a methodology. Economics has never had a more ardent, a more capable, a more illuminating exponent of the philosophical prerequisites of economics than this writer of the inductive "Logic" and of the "Principles of Political Economy."

The questions of the scope and method of social science, and of economics in particular, were settled by Mill who was reared in the atmosphere of Utilitarianism. In essentials he remained true to sensationalism, even though he disavowed Benthamism, and borrowed from Comte in rounding out his logical survey.

Through Mill's Logic the researches of the Naturalists were turned into a science of catallactics. Exactness counted more than comprehensiveness. The aim was to delimit economic investigations and to demonstrate beyond a doubt that social laws were as genuine as those of physics or chemistry. The argument which J. S. Mill used for expounding his theory of deductive economics

cannot here be stated, nor would it be in place. But we may bear in mind that catallactics rested on the supremacy of pain-pleasure sensations and memories and desires over all other psychic states; that wealth was identified with pleasure, and that the hedonistic theory of valuation was as much a part of Mill's logic as of later economic doctrines of price and income. The statical viewpoint likewise presupposed the existence of psychic forces definite and interacting like the physical, so that an equilibrium for economists had to mean a *status quo*, as well as an *average* result. And again: If J. S. Mill defended deduction as the economic method his chief reason was the circumstance, as he believed, that psychological and social happenings obeyed the principle of a Composition of Causes, not of chemical causation. A distinction was made "between the case in which the joint effect of causes is the sum of their separate effects, and the case in which it is heterogeneous to them; between laws which work together without alteration, and laws which, when called upon to work together, cease and give place to others." [2] The processes of consciousness and of society represented a mechanical rather than a chemical blending of elements, and therefore economists were not only permitted, but really compelled, to reason deductively, to consult the basic traits of man for their understanding of the relevant data, and to seek a simplicity of treatment by abstracting dominant motives from the whole of human nature, in short by following the suggestions of Hartley and James Mill who compounded percepts and feelings in a quite arithmetical style out of the primary sensations.

[2] Mill's "Logic," Book III, ch. 6, § 2. The argument is contained in Book III, chs. 6, 10, 11, and Book VI, chs. 4, 7, and 9.

John Stuart Mill, to be sure, was not oblivious of the chemical aspects of physiology,[3] but this he thought somewhat apart from the main point.

Now, though it is true that Historism departed radically from all this kind of thinking on matters psychological and economic, and though it would be wrong to underestimate the force of the Historical outlook as a protest against the individualistic, competitive economics of Smith and the Utilitarians after him, yet on two counts the Historical movement must be regarded as an interlude merely of a larger whole. For in the first place the friends of Historism were in quest of economic laws exactly like their opponents, albeit by a different route, and in the second place the vitality of Utilitarianism was so great that nothing up to the last few decades has seriously undermined it. Indeed, while the adherents of Marginism have naturally magnified their original contributions, in reality the gap between Mill or neo-classicism on the one hand, and Jevons or present-day Marginists on the other, is not unbridgeable. In *fundamentals Marginism resembles Utilitarianism*, the chief differences being the displacement in Marginism of objective terms by subjective ones, and the introduction of a differential, of a margin, whose services were expected to be unique. All that the Utilitarians stood for, to wit, the stress on legal premises of property, freedom of contract, freedom of vocation and residence, mobility of labor and capital in a legal or perhaps technical sense, this and the acceptance of sensationalism as a theory of value and action, or as a basis for an economic methodology—all this the two groups of economists shared in common. The de-

[3] See Book VI, ch. 4, §§ 2-3.

parture from Utilitarian norms was less resolute than imagined. The ties linking Mill and Menger were stronger than the differences of opinion which, to some extent, drove them apart. What characterized both was an insistence upon precise formulations of laws in Price and Distribution, a belief in regularities abstracted in such a manner from reality as to produce a science of catallactics, Mill being inconsistent in this demarcation, because a greater mind and a friend of Comte, while Menger and Marginists in general strove to avoid contradictions regardless of what became of the world of facts.

Remnants of the objective view still exist in definitions and treatments of expenses, of physical supply, and of productivity, but on the whole Marginism was subjective. Psychology, that is, sensationalism and hedonism, proved the bedrock of Marginal reasoning. Man in the center of valuations, psychic states as causes, and last or least units as standards of measurement—such were the innovations that followed upon Utilitarianism. For the rest, everything was as of old. On the continent a transcendental logic served as well as Mill's empirical logic, since both stressed deduction of the formal sort. But otherwise sensationalism formed tacitly or expressedly the substratum of the economic edifice.

Present Critical Attitude.—Nor was any widespread discontent with this state of affairs noticeable before the dawn of the twentieth century. Whatever protests were raised against this or that feature of the orthodox methodology or statement of principles was sporadic. Only as certain changes occurred in the environment and in other fields of scientific inquiry did economists

scrutinize their teachings from another angle, thus arriving by degrees at opinions which now have brought a crisis in economics. We live in a period of transition whose final outcome none can predict.

These changes have been forced upon us largely of course by the abandonment of sensationalistic psychology [4]—of which more in awhile—but the modifications of principles in price, production, and distribution are themselves noteworthy and in part due entirely to a searching of heart among economists as such. Economic literature during the last two decades has excelled in critical revisions and in controversial tone, the Europeans leading in questions of methodology. But the Amer-

[4] For literature in criticism and rejection of the hedonistic psychology see as follows: *Before 1900:* Bonar, J., in *Quarterly J. of Ec.*, October, 1888; Patten, S. N., in *A. A. A. of Pol. & Soc. Sc.*, 1892, pp. 28-29 by way of stressing a complementary-utility law; Stuart, H. W., in *J. of Pol. Econ.*, December, 1895; Powers, H. H., in *A. A. A. of Pol. & Soc. Sc.*, vols. 12-13; Veblen, Th., in *J. of Pol. Ec.*, 1898, pp. 73-97. *Since 1900:* Pigou, A. C., *Econ. J.*, March, 1903; Davenport, H. J., "Value and Distribution," 1904, ch. 17; Veblen, Th., in *Quart. J. of Econ. 1908, J. of Pol. Econ.*, November, 1909; Wicksteed, Ph. H., "Common Sense of Political Economy," 1910; Parker C., in *P. & P. of Am. Ec. Assoc.*, March, 1918, in his "Motives in Economic Life"; Mitchell, W. C., in *Quarterly J. of Ec.*, vol. 29, pp. 1-47, where recent literature on human nature is reviewed; Clark, J. M., in *J. of Pol. Ec.*, 1918, pp. 1-30, and 136-66, a historical résumé; Fisher, I., in his Presidential address before American Association for Labor Legislation, 1918; Hamilton, W. H., in *Am. Ec. Review*, March, 1919, pp. 316-17; Parry, C. E., in *P. & P. of Amer. Econ. Assoc.*, March, 1921, pp. 128-29. For a defense of hedonism see, e.g., Whitaker, A. C., in *Pol. Sc. Quarterly*, 1916, pp. 433-44.

For German literature against hedonistic theory of valuations see: Schmoller, G., in his *Jahrb.*, 1883, pp. 975-94; Simmel, G., "Einleitung in die Moralwissenschaft," 1892-3, vol. I, ch. 2; Boehm-Bawerk, E., "Positive Theorie des Kapitals," edit. of 1909, vol. I, pp. 311-29; Schumpeter, J., "Wesen und Hauptinhalt der Theoretischen Sozialoekonomie," 1908, *passim;* Weber, M., in *Archiv. f. Sozialw. und Pol.*, 1908, pp. 548-54; Cassel, G., "Theoretische Sozialoekonomik," edit. of 1921. For a statement on logical place of psychological premises in economics see, e.g., Wieser, F., in Schmoller's *Jahrb.*, 1911, p. 924, dissenting from Schumpeter.

ican output was but little delayed, and not a whit less pronounced in candor of treatment.

Hardly a tenet of economics, but it has been restated or impugned as to its correctness![5] Not a crucial point in the arguments on price,[6] production, and distribution,[7] but reservations in accepting it have been offered, often with much feeling or aplomb, as if with a premonition of logical implications. Whether price[8] (and factorial shares) are the central problem of economics, whether competition[9] is really the milieu of price-fixing, whether

[5] See for sweeping general criticism: Davenport, H. J., "Value and Distribution," p. ix; Hoxie, R. H., in *J. of Pol. Ec.*, 1906, pp. 337-61; Young, A. A., in *Qu. J. of Ec.*, 1911, p. 424; Haney, L. H., "History of Economic Thought," edit. of 1920, pp. 557, 566; Anderson, B. M., "Value of Money," p. 83; Clark, J. M., in *P. & P. of Am. Econ. Assoc.*, March, 1921, pp. 132-43, article on "Soundings of Non-Euclidian Economics." For noteworthy article on "Extension of Value Theory," see Friday, D., in *Qu. J. of Ec.*, February, 1922.

[6] Fetter, F. A., in *Am. Econ. Rev.*, December, 1920, and Hamilton, W. H., in *J. of Pol. Ec.*, 1918. On refutation of doctrine of a single price see: Watkins, G. P., in *Qu. J. of Ec.*, 1915-16, p. 684; Hoxie, R. H., in *J. of Pol. Ec.*, 1906, p. 425. For a much earlier statement by a French sociologist see Tarde, G., "La Logique Sociale," 1896, p. 365.

[7] Hobson, J. A., "Economics of Distribution," 1900, pp. 16-22, and the same author's "Industrial System," 1910, p. 136.

[8] Padan, R. S., in *J. of Pol. Ec.*, 1904-05, p. 392; Anderson, B. M., "The Value of Money," p. 49; Persons, C. E., in *Qu. J. of Ec.*, 1912-13, p. 547; also: Davenport, in *Am. Ec. Rev.*, 1911, p. 750; Perry, C. E., in *P. and P. of Am. Ec. Assoc.*, March, 1921, p. 124; Stolzman, R., "Grundzuege einer Philosophie der Volkswirtschaft," 1920. Liefmann, R., in *Archiv. f. Sozialw. und Pol.*, 1912, pp. 1-54, and 406-69. On functional correlation of prices see Schumpeter, J., "Theorie der Wirtschaftlichen Entwicklung," 1912, pp. 61 ff, and 166-67.

[9] Stolzman, R., in Conrad's *Jahrb.*, 1918, pp. 303, 1-27, 145-66, 273-304; Anderson, "Value of Money," pp. 570, 559 (also same writer's "Social Value," 1911); Cooley, Ch. H., in *Qu. J. of Ec.*, 1915-16, p. 7; Perry, R. B., in *Qu. J. of Ec.*, 1915-16, p. 464; Davenport, "Economics of Enterprise," pp. 92 *et seq.*; Diehl., K., in Conrad's *Jahrb.*, vol. 51, 3d Ser., pp. 399-426; Zwiedineck, O., in *Zeitschr. f. Ges. Staatsw.*, 1908, pp. 587-654, and for year 1909, pp. 78-128. On idea of an average social value and its rejection see Anderson, "Social Value," esp. chs. 7, 11, 17; Davenport, in *J. of Ec.*, 1906, pp. 143-69;

conventional price-analysis [10] can avoid making a vicious circle because of, or irrespective of, a *status quo* in distribution, whether value is individual or social,[9] whether scarcity [10] rather than utility is determinative of price, what purchasing-power [11] signifies as compared with personal preferences, how expenses [12] figure in price or compare with non-pecuniary costs, what a "factor" [13] in production should signify, how capital [14] is formed and to be used in its several meanings as loan-fund or production-goods or as sheer right to income, whether [15] margins of utility apply to all goods or not, and what

Wieser, F., "Natural Value" (transl. by Malloch, 1893), Bk. II, ch. 3; and Simmel, G., "Die Philosophie des Geldes," edit. of 1907, pp. 476-77, where a Marxian viewpoint is taken.

[10] See Liefmann, R., "Grundsaetze der Volkswirtschaft," 1917, vol. I.

[11] Wright, Ph. G., in *Qu. J. of Ec.*, 1912-13, p. 307.

[12] Simpson, K., in *Qu. J. of Ec.*, 1921, p. 287: "Price approximates bulk-line or marginal cost under normal conditions of competition," gross profits of all producers in a given industry being about 10 per cent of the invested capital. See also Stolzman, in Conrad's *Jahrb.*, 1919, p. 340; Esslen, J. B., in Schmoller's *Jahrb.*, 1918, pp. 1075-1123. On subjective view of cost as economic motive see Liefmann, R., in Conrad's *Jahrb.*, 1913, pp. 603-51 where *Nutzen* minus *Kosten* gives *Rein-ertrag;* Kraus, O., in *Jahrb. der Philosophie,* 1914, p. 45. For a rejection of individualistic cost concept see Haney, L. H., in *Am. Ec. Rev.*, 1912, pp. 590-600. For other discussions of the same topic see Cassel, G., in *Zeitschr. f. Ges. Staatsw.*, 1901, pp. 68-100; Clark, J. M., in *Qu. J. of Ec.*, 1913-14, p. 770; Bell, Sp., *ibidem,* for year 1918, p. 523; Knight, F. H., in *J. of Pol. Ec.*, 1921, p. 317.

[13] Cassel, G., "Nature and Necessity of Interest," 1903, pp. 74 and 85. See also Davenport, "Economics of Enterprise," ch. 22.

[14] Tuttle, Ch. A., in *Qu. J. of Ec.*, 1904, pp. 54-96; Borght, R. v. d., in Conrad's *Jahrb.*, 1903, pp. 596-607, on capital as a loan-fund. See also Veblen, Th., in his "Theory of Business Enterprise," 1904, chs. 5-6; Davenport, "Economics of Enterprise," ch. 18; King, W. I., in *Am. Ec. Rev.*, December, 1920, p. 754; Friday, D., in *P. and P. of Am. Econ. Assoc.*, March, 1919; Moulton, H. G., in *J. of Pol. Ec.*, 1918, pp. 484-508, 638-63, 705-31, 849-81, and the same writer's "Financial Organization of Society," 1921, ch. 10.

[15] Wieser, F., "Ursprung und Hauptgesetze des Wirtschaftlichen Wertes," 1884, pp. 198-200; Flux, A. W., "Economic Principles," 1905, p. 23; Watkins, G. P., "Welfare as an Economic Quantity," ch. 9; Haney, "History of Economic Thought," 1920, p. 568.

their rôle as price-determinants, whether incomes [16] are prices or not, whether *one* law [17] governs all prices of goods and services, what the number of sharers in the distributive process,[18] how productivity [19] as against valuations or impatience fixes certain shares—such and other questions were raised anew of recent years, the answers varying so greatly that it would be the height of complacence, of lazy indifference, to present economic science as having attained a secure foundation.

The wave of criticism has swept the whole western world, not merely one country or one school of economists. The crest of the wave may have passed, in the eyes

[16] Hobson, "Economics of Distribution," 1900, p. 76, and the same writer's "Industrial System," 1910, p. ix and 274. See also Davenport, "Value and Distribution," p. 274; Anderson, "Value of Money," pp. 83, 112, 119; Veblen, Th., in *J. of Pol. Ec.*, 1909, pp. 620-36, and for 1908, pp. 147-95; Carlile, W. W., "Monetary Economics," 1912, chs. 5-6; Englaender, O., in Schmoller's *Jahrb.*, 1920, pp. 399-450, 709-39; Stolzmann, R., in Conrad's *Jahrb.*, 1918, pp. 1-27, 273-303.

[17] See Simiand, F., "La Methode Positive en Science Economique," 1912; Tugan-Baranowsky, "Soziale Theorie der Verteilung," 1913; Hobson, "Economics of Distribution," 1900, ch. 10; Davenport, "Economics of Enterprise," in discussing shares versus costs of production; Cannan, E., in *Qu. J. of Ec.*, May, 1905. For a general disavowal of distributive analysis up to date see Fisher, I., in *P. and P. of Am. Ec. Assoc.*, March, 1919, p. 11. For a defense of classicism in this matter see Kleene, G. A., "Profit and Wages," 1916, ch. 9.

[18] Clark, J. B., "Distribution of Wealth," 1899; Hollander, J. H., in *Qu. J. of Ec.*, 1903, pp. 261-79; Schumpeter, J., "Wesen und Hauptinhalt," p. 390; Kellenberger, in *Zeitschr. f. die Ges. Staatsw.*, 1912, pp. 658-70.

[19] Davenport, "Value and Distribution," p. 471; Adriance, W. M., in *Qu. J. of Ec.*, 1914-15, pp. 149-76; Hobson, in *J. of Pol. Ec.*, 1903-04, pp. 449-72; Aftalion, A., in *Revue d'Economie Politique*, 1911, pp. 145-84, 345-69. See also Parker, U. S., in *J. of Pol. Ec.*, 1907, pp. 231-37; Davenport, "Value and Distribution," p. 364; Kellenberger, E., in *Zeitschr. f. Ges. Staatsw.*, 1912, pp. 658-70. For controversy on determinants of interest-rates see, among others, Davenport, "Economics of Enterprise," p. 380; Knight, F. H., in *Qu. J. of Ec.*, 1915-16, pp. 298, 310; Brown, H. G., in *Qu. J. of Ec.*, 1912-13, p. 650; Veblen, "Instinct of Workmanship," 1914, p. 47, note. See also McGoun, A. F., in *Qu. J. of Ec.*, 1917, pp. 547-70.

of some, but indications are not altogether to that effect. What is more, there are outward signs of revolt, of dissension or despair that must attract all those engrossed in economic speculation. For one thing recent literature everywhere, though voluminous and suggestive in particulars, has carried on no consistent development of the main body of doctrines. For another thing, the study of business life, of cycles [20] of production and profit, has gained greatly at the expense of erstwhile static surveys, a tendency that may be welcomed, no doubt, but none the less provides food for thought if one is familiar with past preachings and the possibilities of the future. In the third place, colleges in America have of late favored an emphasis on description rather than on a teaching of laws, so much so in places, that one wonders whether the science of economics is held worthy of serious cultivation or not. The demands of business have crowded out of the class-room the urge of a quiet, contemplative, non-utilitarian diagnosis of events, and what is widely preferred is a near-by, practical acquaintance with the competitive norms ruling our producers. And, finally, there is reappearing among us a political economy whose pri-

[20] The literature on business cycles, especially from a statistical standpoint, has grown greatly in the United States, as elsewhere. Among American books on the subject may be mentioned these: Jones, E. D., "Economic Crises," 1900; Burton, Th. E., "Crises and Depressions," 1902; Hull, G. H., "Industrial Depressions," 1911; Moore, H. L., "Economic Cycles, Their Law and Causes," 1914; Mitchell, W. C., "Business Cycles," 1913; Bilgram, H., and Levy, L. E., "The Cause of Business Depressions," 1914. Noteworthy are also the investigations of special committees maintained by large industrial plants and banks for purposes of business forecasts and such organized, strictly scientific, efforts as those of the Harvard University Committee on Economic Research in Cambridge, Mass., and of the National Bureau of Economic Research Incorporated in New York City.

mary aim is national development on partly collectivistic lines, it being sometimes openly conceded that price and income laws are too hypothetical or unpractical or vague or variable to deserve attention as much as descriptive accounts and precepts for political application.

Now, this world-wide ferment directed against orthodox economic principles is bound to engage our solicitude. One is prone to ask: What is wrong with traditional doctrine regardless of its premises in psychology, logic, or environment? To what extent has the analysis of price and shares, which has so preoccupied us for nigh a hundred years, failed in accomplishing its declared purposes, in yielding the regularities or generalizations that are the boast of all science? What may be said about the psychic facts alleged to be back of pricing, no matter whether we cling to sensationalism or not? Are the terms demand and supply quite clear? Were they used so as to give us a self-consistent view of the pricing mechanism? How should our notion of a *determining* factor in a causal sense be shaped, and what do we mean by *fixing* or measuring prices?

Or, again, suppose we start with the definitions of catallactic economics, how are they logically related, and what is involved in our coloring them individualistically or in terms of a pecuniary norm? What has the division of production to say about the line of approach suitable to distributive problems? Are incomes prices in all respects, and if so, what prompted economists to overlay their price analysis with considerations of other elements that consumption goods appeared to be free from? What *are* the laws of production, and how much have they told us? And as for the margins so conspicuously

paraded by the latest subjectivistic group, what have they done for us that might make them an indispensable feature in economic theory?

Of these and other questions the critic will be disposed to say something, even if he were absorbed simply in an estimate of the principles of economics in the narrower sense. But of course, his work will not end there; certainly not nowadays where so many fundamental tenets have been assailed. The question in fact is: Why these changes of opinion? Whence the change in venue that is threatening not only the supremacy, but the very life, of Utilitarian and Marginal economics?

General Grounds of Critical Attitude.—As a first guess, to be sure, one may point to the new world of actualities in which we live, and make it seem as though the changes wrought since Adam Smith or John Stuart Mill demand inexorably a readjustment of theory. It is true undoubtedly that these transformations have affected the views of the closet philosopher and of the professional economist, thus accounting for a fraction of his complaints or pleas of reform. The legal premises of three generations ago are not so completely realized to-day. Competition and freedom of contract have been put under restraint. There are powerful reasons why we should take the assumptions of the classicists with a grain of salt, even if mindful of the rough accuracy of the rules of procedure they laid down. Competition still obtains, but not as much among individuals as before. The fighting unit has been enlarged, so to say. We compete as groups rather than as single buyers or sellers, producers or consumers. Mobility of the technical sort has been circumscribed or even annulled by conditions

that our forefathers knew nothing of. Sentiment has in no small measure gone against an untrameled individualism, and thinkers there are to-day more than ever who deplore it on the one hand, or pronounce it fictitious on the other. Thus economic principles have gradually undergone a censure akin to that brought to bear upon the practices of the egotistic entrepreneur.

Yet, if we seek for the main key to the riddle that confronts us we must reach out farther and reckon with facts that are not altogether economic. We must remember the changes in other spheres of inquiry, in psychology, biology, logic, and philosophy in general. We must prepare for a long journey of exploration in order to orient ourselves finally in our own precincts of Principles of Economics. Economics was first founded by men who were philosophers even more than economists, whose training fitted them peculiarly for giving social happenings a deep, perhaps even an occult, significance. Let us not forget that Smith and Quesnay and his ilk, Bentham and James and J. S. Mill, and Say and Sidgwick and Jevons were minds of large caliber, thinkers whose greatest desire was a discovery of laws comparable to those of natural science, though socketed in certain presuppositions metaphysical. Philosophy is the mother of all sciences, and to this rule economics is no exception!

Put differently, Utilitarian and Marginal economics have their roots both in theories of human nature, and in theories of knowledge. If catallactics eventually supplanted the theory of prosperity of the Naturalists the reason lies in a set of axioms and speculations that only during the last few decades have been, in large part, definitely repudiated. Statics and the "mathematical

method," price-mechanism and laws of income, valuation and productivity—all of these hinge on ideas framed not by the economists primarily, but by outsiders, by abtruse thinkers in alien fields. If, then, changes occurred in these realms of thought, economists were likely to be touched by them sooner or later. The developments in psychology especially had echoes in economic literature. So it is fitting that we give them some consideration, even though they cannot solve our problem entirely. What is certain only is that psychology, which furnished all-important premises to economics, has made marked progress since the classics were penned, thus forcing eventually a new confession of faith from economists now living.

In outline the progress of psychology was something like this.

Developments in Psychology.—Broadly speaking modern psychology was at the beginning a part of either the rationalistic or the empirical systems of philosophy. With Descartes Rationalism, not only as a theory of knowledge, but also as an inquiry into psychic processes in the narrower sense, became frankly dualistic, positing "faculties" and innate ideas as a key to the understanding of human nature. Reason was exalted and mind sharply set off against substance or extension. Men like Tetens and Wolff in the eighteenth century popularized the notion of distinct departments of consciousness, the former giving currency to the threefold division of psychology into Cognition, Affection, and Volition.

The question whether causality obtained in the psychic realm was immediately raised, but never answered to the satisfaction of all. The Rationalists, and their descend-

ants in one respect or another, clung to the distinction between causal connection and a freedom of the will, modes of consciousness thus being expressedly or implicitly contrasted with physical facts. Personality was the active agent and ultimate reality. The mind was superior to the objects it recognized in the outside world. The metaphysical background of psychology, in other words, was never lost sight of, although for practical purposes it did not seem so important whether psychophysical parallelism or interactionism served as a postulate. This was one of the by-products of the Rationalistic viewpoint, which ever since has made enemies of two groups of investigators, viz., those who accepted causation as universal, and those who preferred to leave a gap between psychics and mechanism.

At the same time, however, that Greek speculations on soul and mind were continued by continental philosophers, British thinkers developed no less zealously the empirical standpoint; and it was through the ascendancy of this latter that social science advanced rapidly even before the nineteenth century.

Empirical psychology was at first sensationalistic. Sensations as basis of all consciousness and knowledge were expounded vigorously, from Thomas Hobbes on. Associations were used to explain thought, reasoning, and policies of the individual. The motives of all men had an intellectual origin due to the close dependence of ideas and memories upon feeling. In Germany a similar psychology was fostered by Herbart, somewhat in opposition to the older faculty theories, although on the other hand it goes almost without saying that Herbart was not an out-and-out empiricist. He might speak of idea-

forces and follow Bentham in balancing psychic states like physical forces, but none the less the Kantian influence was noticeable. Apperception took the place of the simpler composition of ideas preached by Hartley, Hume, and J. Mill, while the transcendence of the soul, of a personality safely protected from all irreverent pryings of the scientist, went as a matter of course.

Not Herbart, but the experimental methods introduced by Weber, Fechner, Helmholtz, and their contemporaries elsewhere transformed speculation into a science. Physiological tests and the use of the microscope in studying organic matter helped to put psychology on a solid footing. Sensations were measured relative to increases of stimulus, and functions emphasized to the disregard of an old-time structural presentation of the mind. The ground was thus prepared for a broader, unified view of human nature which the evolutionary view of life forced irresistibly upon psychologists no less than upon biologists. Phylogeny displaced or complemented ontogeny. The truth that all things are relative and most things destined to render specific services took hold of investigators everywhere. The biological interpretation indeed threatened to uproot completely the psychological one.

As a result of this change of venue reason was minimized and the irrational side of man magnified. Some turned to feelings and the emotions as the substratum of human action and thought. Others learned to recognize in the instincts an instrument for survival equal in power and significance to the faculty of reasoning, which heretofore had attracted so much attention. That nerves formed an indispensable prerequisite to a learning proc-

ess was not denied, but that certain inborn conative dispositions guided it seemed also clear. Whether instincts were the distinct, definable units, classifiable as easily as was once believed, is now a moot point. The trend of recent years has been toward a more reserved pronouncement on this vexing problem, especially since the physical basis of heredity and the principles dependent upon it have been more fully revealed. But nevertheless the functional, genetic view of psychological facts has gained rather than lost prestige.

What is more, a number of psychologists have resolutely turned their back on the soul aspects of human nature, emphasizing instead of physiology the internally or externally manifest expressions of thought, feeling, and will. Thus Behaviorism virtually rejects consciousness, satisfying itself with physical facts and outward forms subject to observation and measurement. Closely related to these mechanistic Behaviorists are those who see in glands and their secretions (notably the ductless glands), in muscle, in blood pressure and the autonomic system, the key to man's conduct and moods. Neurology thus has forfeited its preëminence among psychological data, while in the numerous agents of metabolism human thought and action are held to become alone intelligible. One branch of contemporary psychology thus leans unmistakably toward a materialistic interpretation of life.

Yet again psychology has likewise developed in an opposite direction, as the vogue of the Freudian philosophy proves most strikingly. Instead of self-consciousness and physiological causation these investigators concern themselves with the unconscious or, as regards a

minor group, with the subconscious. Instead of chapters on will, memory, sensations, and concepts we find discourses on error, associations, wish, traumatic fixation, libido, and dual personality. Psychophysical parallelism is replaced by an interactionism that accords to psychics as genuine a causal bearing upon physical things as had always been assumed between physical things themselves. Thus associationism has again assumed importance. Thus dreams and wit and humor and a large variety of neural disturbances have arrested the attention of specialists. And thus psychology is to-day studied from more standpoints than ever before, introspection holding its own in competition with experimentation. On some fundamentals most psychologists are in agreement. But with regard to others dissension is widespread and ever ready for a hearing before an interested lay public.

Meanwhile, however, the *applications* of psychology have not been wanting nor waiting. On all sides suggestions have been made and carried into fields quite distinct from psychoanalysis in any sense. Thus for one thing research since the days of sensationalism has led to new theories of esthetics and ethics, or what some like to call ethics. It was natural that our ideas of the beautiful and of right and wrong should change with our understanding of pleasure or pleasantness, of will and motive, affection and emotions, instincts and attention. Logic also was deflected from its old path and widened so as to include phases of inference that formerly appeared negligible or nowise "logical." What knowledge is and to what extent we may possess it, this became a new query to be dealt with according to modern psycho-

logical values. Educators proposed to use the elements of memorizing, of interest, and of reasoning for aiding the youths of the country in our lower and higher institutions of learning. Psychoanalysis accomplished much by ignoring parallelism and treating disease as a sequel to psychic disorders, to "lost" memories, or to inhibitions foisted upon mankind by a tyrannical social taboo.

The social sciences, too, benefited by the march of psychological experts. Sociologists could not turn a deaf ear to them because they were concerned with the origins and the rôle of the *mores*, with a dialectic of cultural growth, with revolutions and with wars, with crime and vice, religion and race traits, and so on; economists, while not as versatile as their colleagues in sociology or history, nevertheless had reason to watch the turn of events, since their premises had always been psychological in large part, and because of their exposition of the problem of price and income, of consumption and productivity.

Results of Breakdown of Sensationalism.—Thus, in fine, the increasing criticism leveled against economic principles since 1900 took much of its strength from the later contributions of psychologists. Since hedonistic associationism had broken down, economics abandoned a corresponding theory of valuation and motivation. The exact connection between traditional economic psychology and the Utilitarian (classical) or Marginal price-income theorems was never made clear. It remains to-day for the critic to show in detail why the value-analysis of economists is faulty, and how far we must swerve from beaten paths before attempting further progress. But it is well understood by many that the downfall of sensa-

tionalism has somehow necessitated a turn-about in economic theory. Indeed, we may ask, is it possible that so basic a premise as the pain-pleasure calculus, as the intellectualistic view of the affections, can be abandoned without its reacting disastrously upon other assumptions in economics? Is it not natural that catallactics and statics should suffer from this recantation of creeds? May we expect the monism of the Naturalists to hold sway hereafter, in spite of the newer psychology, in the face of all dualistic epistemologies promoted since Kant, and professed by men in one guise or another? Or to put a still different question: Should economists slough off their old beliefs without anxiety, either because contemporary psychology will provide them with better data, or possibly because no psychology whatever is needed? Are no premises of any kind needed? Some will ask this. And we know of those who have answered in the negative because they wish economics to remain a science on its own merits.

Such a simplification however may always be challenged, since the data of economics are not verifiable exactly like those of physics or chemistry. There is a difference between social and natural sciences that we cannot afford to ignore. To appeal to facts is natural enough, but it will depend upon the kind of subject-matter dealt with whether the appeal can be followed up, or not. Economics for the most part has not been a factual science whose results could be tested with ease. The conclusions which our principles of economics embrace have with rare exceptions not been of the kind that, for instance, chemists treat of. A difference exists between those two classes of inquiry that gives sanction

to our driving the critique of present economic teachings back of legal or psychological postulates.

Looked at from one point of view, of course, all history is no more than a recurrent confession and abnegation of faiths. We note, in perusing the annals of human life, regularly ascending and descending curves of achievement; epochs in which beliefs are formulated and zealously defended as shining truths, and others in which the scoffers have the best of the situation, not so much because they are superior judges or more effective promulgators, but because in the light of new interests and environmental, physical or cultural, data they are able to make out a better case for both the familiar and the strange. We do not have to accept a particular variety of an economic interpretation of history in order to recognize the intimate relation between conditions and creed, or between either and conduct. It is not to be doubted that views change, and that most of what does not appeal immediately to our senses has a variable content, a meaning differing with place, period, and people. There *are* long stretches of a development of ideas, and relatively shorter ones devoted to refuting what was developed. That which was self-evident at one time, becomes perhaps incredible the next time. What once elicited the praise of the most competent, is later stigmatized as fantastic or wholly misleading. Men for reasons not now of importance do love contradictions and contrasts, speak in hyperboles, and swing continually from one extreme to the other. Ardor now, and apathy soon afterwards! One age building, while the next tears down with jubilation. Dogmatism followed by skepticism, and avowals eternally oscillating, as if stability were an impossibility!

An intellectual see-saw, as a great philosopher has taken pains to tell us; a process of thesis, antithesis, and synthesis whose cycles and courses testify eloquently to the foibles and fatuous ambitions of mankind.

All science, then, is relative and subject to periodic revampings. Yet, while this is true, it must be admitted that the natural sciences have been much more successful in establishing a lasting body of truths than philosophers or even social scientists. No matter what the modifications of scientific creed, a residuum of indisputable facts has always remained to provide inspiration for further endeavors. For the most part the students of nature phenomena have been in the enviable position of being able to say to the doubter, to the enlightened public: If you believe not, find out for yourself. Here are data for you to work with; here are instruments for a testing; here are assured facts relative to which your subsequent generalizations must take shape. See what you can do with them, or prove by way of addition.

Now, this challenge which most natural scientists may hurl at their skeptical opponents of honest intentions, and the grounds of which are themselves an explanation for the agreement usually characteristic of experimenters —certainly as regards fundamentals of fact, this sort of challenge is not popular among economists because the nature of their subject matter, of their methods, and of their generalizations forbids it, precludes it. Economics in particular has almost from the outset relied upon sheer assumptions, or worked with data whose service as assumptions in economics is now no longer cherished. Social sciences do not deal with visible, weighable, tangible facts. They cannot, generally speaking, take ref-

uge in experimentation when doubt arises. They must make their peace with what postulates are newly offered, or build on different lines if the foundations go to ruin. In short, the appeal to verifiable facts and to the adequacy of the deed of investigation lacks force because the facts themselves are at issue.

Essentials of an Economic Methodology.—From time to time economists therefore, like other students of human events, have stated the logic of their science in a more or less gratifying manner. A few of these essays have sufficed for their age, but the most of what now ranks as economic methodology takes little account of recent problems and requirements for growth in research. The principal writers on the subject have accepted formal logic for their view of deduction. They have opposed induction uncompromisingly to deductive inquiry. Specific causation and the canons first announced by Mill in thoroughgoing fashion have been given undue prominence, to say nothing of their abuse by some. An inter-group debate on Historical versus deductive methods has accentuated the shortcomings of each, but without hewing straight along the line sketched out by the logicians themselves. We have heard of the contentions between the Historians and the classics in England, of the reason why a generalization from past occurrences must be tardy in coming, or be inconclusive. Definitions have been fully discussed and tabulated, and the premises stated on which a static economics rests. All this has been done more than once, and in addition we have the incomparable, the epoch-making, the never-to-be-overrated work of J. S. Mill, in which for the first and only time the logic of catallactics was expounded at length,

with all the cogency of reasoning and range of information that its author had at his command. But one may ask: Is it not reasonable to expect further counsel from logic since the publication of Mill's work in 1843? Should we be satisfied with comparing schools of economic theory, examining with lingering fondness their definitions and use of abstractions? Or may anything definite be said on the present question of what economics must be without sensationalism, without possibly any psychology, and what it may undertake to do now that our notions of causation, of laws of nature, of inference and of human nature in general have weaned us from the Enlightenment?

A logic of economics evidently must be much more methodology than economics. It is not the latter which gives exceptional content to the former, but the former which dictates to the economist. If certain things constitute logic or inference or scientific method or law or human knowledge, then for any one field like economics certain other truths follow. The basis of methodology may be several things, for we touch here upon last questions not amenable to our five or ten senses; but once we have stated its essentials the corollaries for some one field like economics will be evident enough. Whether the epistemologist takes counsel with psychologists, whether the logician learns much or little from the actual routine of natural sciences, this may be a moot point; but there can be no doubt that economics, especially because it has been so far a conceptual science dealing with abstracts and not with the events as they happen from a common sense viewpoint, had to acknowledge its obligations to philosophy. What its basic definitions should be, what

the nature of its methods and conclusions, or to what extent it might make sure of its metes and bounds, these questions were inevitably settled by, first the premises in psychology, and, secondly, the accepted views in logic and epistemology.

Recent German Economics.—The close affiliation between economics and a general methodology might, à propos of this topic, be well illustrated from what happened in Germany, the home of Kant and a bulky literature on all things theoretical. Not that other countries failed to take cognizance of this underlying problem. Not at all. For in Great Britain, France, and Italy much was written in late years on the philosophy of economics, preferably in the light of changes in psychology. Everywhere, the United States not excluded, a subjectivistic attitude gained the upperhand. The pragmatic, phenomenalistic tide that swept over Anglo-Saxon lands, was symptomatic of what a modern democracy wanted, of what must occur when problems of knowledge are attacked in psychological laboratories. But when all is said and done we must grant the leadership of German economic methodology since 1900, as indeed it remained uncontested in both metaphysics and logic.

In Germany, then, the revulsion of feeling that followed the triumph of scientific materialism precipitated lively debates on the limits of all human knowledge, and of natural science in particular. Vitalism and Fichteanism, Neo-Kantism and Voluntarism [21] were samples of

[21] For typical statements on this subject, by Voluntarists mainly, see: Muensterberg, H., "Philosophie der Werte," 1908 (e.g., pp. 20, 141) and its American version as "Eternal Values," p. 155; the same author's "Psychology," 1914, especially chs. 2, 21, 24; Stein, L., "Philosophische Stroemungen der Gegenwart," 1908, p. 341; Rickert,

the movement which tried to reinstate idealism after evolutionism had failed to answer all queries. It was preached once more that monism, unless it be a transcendental idealism, cannot satisfy the human craving; that between mind and matter there is, empirically viewed, an unremovable chasm; that will is one thing, and law another; and that man either valuated events, in which case he ceased to be a scientist, or described them simply in an objective manner, in which case he could never treat of *social* phenomena. The difference between a knower and something known was again made clear. The dual aspect of all knowledge was illustrated in a hundred ways. Arguments were advanced not only for separating ethics from science, and the purposes of the philosopher from those of a scientist, but what is more, sciences themselves were classified according to whether they dealt with psychical and physical data, or with historical or non-historical facts. Ethics, consequently, was put aside as something *sui generis*, and this irrespective of whether its roots were traced in biology and principles of the learning process, or in metaphysics pure and simple. But in the second place, it was contended by many [22] that law and causation do not obtain in

H., "Grenzen der Natur-Wissenschaftlichen Begriffsbildung," 1902, and his "Kultur- und Natur-Wissenschaft, edit. of 1915. For a criticism of Rickert see Muensterberg, H., "Philosophie der Werte," where *all* sciences are treated as value-judgments; Becher, E., "Natur-Philosophie," 1914; and Schmeidler, B., in *Annalen der Natur-Philosophie*, 1904, pp. 24-70. For other treatments of science more or less from an axiological standpoint bearing on a classification of sciences, see: Windelband, W., "Einleitung in die Philosophie," 1914, Part I, § 12; the same writer's "Logic," in "Encyclopedia of Philosophical Sciences," 1913, vol. I, pp. 48-9; and particularly his "Geschichte und Naturwissenschaft," 1894.

[22] For instance in *Zeitschrift f. Sozialw.*, 1910, five articles by Pohle, L., and in Conrad's *Jahrb.*, 1903, article by Bunzel, G., pp. 433-91.

socio-economic events, or that at any rate a vital difference existed between an historical and an economic-scientific approach to human affairs.

The distinction between a study of individual facts and classes of events was, to be sure, an old one. At bottom it was Kantian and could not be escaped, once one opposed a critique of pure reason to a critique of practical reason. Furthermore, Schopenhauer [28] had called attention to the difference between the work of the historian and that of science, though this seems to have been forgotten. But certainly this cleavage-line was drawn again with greater nicety and elaboration of arguments, the economists, beginning with Menger, using it as a weapon against the Historical school or against the claims of the Verein fuer Sozial-Politik. Thus, in the end, a host of methodological questions were resuscitated and by most of the writers answered in favor of a causal basis of economic laws, statistical methods gaining by the decision, while the right or power of psychologists to guide the economist became doubtful. To a degree therefore Voluntarism did for German economists what the latest doctrines in psychology did for economists in America and England: It pointed a way for the abandonment of eighteenth century sensationalism and forced a decision on two questions, viz. first, whether economics should seek to get along without any psychology, and secondly whether it was a science of causal relations capable of being formulated into laws. In spite of its metaphysical origins economic methodology had thus assumed new definiteness, breaking at certain points with an older logic.

[28] Schopenhauer, A., "World as Will and Idea" (transl. by Haldane and Kemp, 1891), vol. 3, ch. 38.

The Methodological Question.—There is, however, another way of stating the problem as economists to-day must face it. It is not necessary that we commit ourselves at once on the difference between historical and non-historical sciences, or between law or causation on the one side, and willed acts on the other, or between mechanistic and telic norms of empirical data. Such topics may fitly form a part of our discussion, if we wish to exhaust our subject: but a briefer way would be to remember that much of the traditional methodology of economics rests on that very psychology which now few deem worthy of serious consideration. Sensationalism, a formal logic handed down from the Middle Ages, and a theory of induction whose Canons J. S. Mill has given widest currency—these are the backbone of Utilitarian-Marginal methodology. Catallactics, as already remarked, was derived from this hedonistic philosophy. Ethics was in most cases allowed to be a metaphysical, and not a scientific, problem. The delimitation of sciences rested principally on a grouping of things perceptually apprehended; while applied economics, so far as *laissez faire* had any room for it, was based not only on the laws of consciousness and behavior known to sensationalism, but in part also on certain physical facts which the economists retained within their survey, catallactics notwithstanding. As to method, we need not repeat that the deductive method connected closely with the subject matter of formal logic; that statics owed most to sensationalism; and that statistics at the time was so much in its infancy that it could scarcely be mentioned as a distinctly useful, not to say essential, device for economists. Whatever was most important in the

orthodox methodology (by which is meant here that of classical, neoclassical, and marginal economics) took its main support from sensationalism and formal logic. The remainder, while of import, took a secondary position.

Now, if economics is to become more strictly than heretofore a factual science, reducing abstractions to a minimum and taking the world in substance as it is, the key to an economic methodology will lie, not in rules of deduction prescribed by the logician, or in principles of association and of ideation *à la* Hartley or James Mill, but in modern psychology, in the observation of what science actually does to obtain its generalizations, and in a careful analysis of law and causation free from all historical bias. (Submit your views on law and causation, and you have decided other questions pertaining to the methodology of any science.) Explain the nature of inference, particularly comparing it with the measurements of science, and you have added further materials to the building of an economic methodology. The answer as to what is acceptable if eighteenth century notions are to be repudiated is given by the examination of these matters just alluded to. Epistemology and logic invariably must determine the character of a methodological creed. Whatever aids they may invoke in stating their case will prove consequential for the student of methods.

If then it is borne in mind that a law of nature has a subjective aspect as well as an objective, and that laws are statements of *elements* united in time or space as well as of relative *quantities* of such elements, the limits of scientific knowledge will be sufficiently comprehensible. It will be part of our task to emphasize the selective

principle governing the formulation of laws, to compare different sorts of units involved in these laws, to distinguish between personal and epistemological subjectivity, to state fully the difference between quantitative and qualitative relations, and thus to establish a line of demarcation between broad groups of sciences as well as between, possibly, economics and its allied fields.

What is the difference, if any, between a law and a correlation? This is a not insignificant question to be answered. What are the variables that all sciences deal with, and what those peculiar to organic existence, thanks to which especially the work of social philosophers is made so arduous and, in a sense, unsatisfactory? What will be our preference—a deductive-reflective method or a statistical investigation—if we view the variables of economics as complex units incompletely known, to be correlated only within vague units of space and time? What exactly is responsible for the instability of socio-economic data and generalizations, and how are we to link up such events with the facts studied by psychology? And again, what follows for the scope of economics, for applied economics, and for ethics, if a genuinely realistic, yet dualistic view is taken of law and causation, or of human knowledge?

As to ethics, is an empirical attitude reconcilable with its exclusion from economics, or is there a real point of contact between the two? By what route are we to arrive at moral judgments without conjuring with transcendental concepts?

If it is meet that any science be bounded theoretically, independent of what the results of research seem to demand or not to demand, must it be via other sciences, or

through a proper understanding of law and causation? On what grounds, for instance, may we classify sciences or set bounds to any one as a basic or special science? What must govern our delimitation of economics, either relative to other spheres of inquiry, or without reference to them directly?

How far is it logical to speak of an "applied" economics if we remember the nature of scientific law and of the dialectic of mind and human progress? Should government be treated as an integral part of economic data, or does it stand aside as a benefitee, like the engineer who exploits the constants of a physical environment? Is it a case of science *versus* art, or should we be more eager to acknowledge the transiency of any public policy whatsoever, emancipating ourselves from a sociological appeal?

The contradistinction between causation and volition which modern philosophers have often made is not of course without force or practical value. But can we cling to the older hope of detecting the exact cause? Or rather, shall we be sure of finding even approximate causes, if laws are qualitative as well as quantitative expressions? What follows if we recognize the selective grounds of a law of nature, its hypothetical values? What does the modern view of causation add to, or subtract from, particularly that of the British empiricists who in J. S. Mill's Logic [24] perfected Canons of Induction in a most positivistic spirit? That cause and effect are not what a common sense man would guess has long been admitted. But apart from that, should we accord to causation anything whatsoever that is not given in the

[24] Book I, chs. 1 and 4.

idea of a law? And if we revise our idea of causation in the light of prevailing conceptions of scientific knowledge, can we continue to use a theory of imputation, as economists have done, implying the possibility of measuring factorial shares, nay, of specifying all the determinants of values?

The skeptical attitude however which, it may appear at first sight, is thus cultivated with regard to fundamental tenets in methodology, cannot deter us from granting to measurements a central position in any scientific realm. On the contrary, it goes without saying that measurement must be separated from inference as such, and that both together constitute science in its functional aspects. But how shall we interpret the words deduction and induction which have for so many years figured in economic methodology? Is deduction to be taken in the formal logical sense or not? Is the one operative exclusive of the other or not? Do we mean by a deductive economic method what the logician means by deduction or syllogistic reasoning? Is proof in either case the same as verification, and must statics be necessarily a deduction of conclusions from select formal premises?

The mathematical method, as everybody knows, has often been differentiated not only from the inductive, but also from the introspective, procedure of professional philosophers and of other inquirers.[25] But what is our

[25] The following notably: Eulenburg, F., *Archiv. f. Sozialw. u. Pol.*, 1911, p. 767, 1905, pp. 519-54; for 1910, pp. 711-78; for 1911, pp. 689-780, e.g., p. 747; Haas, A., in Schmoller's *Jahrb.*, 1917, vol. 41, No. 2; Weber, M., *ibidem*, for 1905, pp. 1323-84, where the logical problem of Historical economics is reviewed; Weber, M., in *Archiv. f. Sozialw. u. Pol.*, 1904, article on "Objektivitaet Sozialwissenschaftlicher und Politischer Erkenntnis; Kistiakowski, Th., "Gesellschaft und Einzelwesen," 1899; Vierkandt, A., in *Zeitschr. f. Sozialw.*, 1912,

reason for this distinction which, by implication, endows mathematics with peculiar virtues? What can this latter really do that the standard methods of science, or inference as such, cannot accomplish? Is the proof of mathematicians an end comparable with that of scientists? Or is there need of emphasizing the difficulties in the way of making something *true*—difficulties which the syllogism very properly brushes aside? Manifestly, it must be in stating the difference between *formal* deduction and the deductive thinking of *science* that economists will learn how to interpret their own reflections. But how should they then contrast induction with deduction?

Again. Statistics has been called both a science and a method.[26] Its beginnings, in one sense, antedate the founding of economics, but in another they are much

two articles; Spann, O., *Zeitschr. f. Ges. Staatsw.*, 1908, pp. 1-57. See also Schumpeter, J., "Wesen und Hauptinhalt der Theoretischen National-Oekonomie," 1908, Preface, and pp. 28, 37, 58, 105-07, 118. For psychology as basis of economics see: Wundt, W., "Logik," 2d edit., vol. II, Part 2; Ruemelin, G., "Reden und Aufsaetze," vol. I, 1867: "Ueber den Begriff eines Sozialen Gesetzes"; and Neumann, F. J., in Conrad's *Jahrb.*, 1898, pp. 19-20. For writers denying the existence of real causal socio-economic laws see: Stammler, R., "Wirtschaft und Recht nach der Materialistischen Geschichtsauffassung," 1896, and the same author's "Lehre von dem Richtigen Recht," 1902; Janssen, O., *Das Wesen der Gesetzesbildung*, 1910, pp. 221-23; Stolzmann, R., "Grundzuege einer Philosophie der Volkswirtschaft," 1920; Biermann, W. E., in Conrad's *Jahrb.*, 1904, pp. 592-624; and notably Gottl. F. von Ottlilienfeld, in *Arch. f. Sozialw. u. Pol.*, vol. 23, pp. 403-70; vol. 24, pp. 265-326; and vol. 28, pp. 72-100. Gottl. takes a middle ground, but leans favorably toward Rickert's view of science and history.

[26] For discussions on statistics as *method* see: Sigwart, C., "Logik," vol. 2; Lexis, W. (who made valuable contributions to the statistical theory of induction), in Conrad's *Jahrb.*, 1879 and 1886, and his "Abhandlungen zur Theorie der Bevoelkerungs und Moral Statistik," 1903. For men considering statistics an independent science see: Ruemelin, G., "Reden und Aufsaetze," vol. I, 1875; Mayr, G., in *Allgemeines Staatswissenschaftliches Archiv.*, vol. 11, 1918-19, pp. 1-50; Seutemann, K., in Schmoller's *Jahrb.*, 1913, pp. 1-36. See also Wundt's view in his "Logik," 2d edit., vol. II, pp. 523-27.

more recent. So far the demand for a statistical study of social data has not been urgent, partly because of the premises on which catallactics was built, and partly because the means and methods of the statistician had not been thoroughly understood in earlier days. Yet to-day statistics must be assigned a definite place in any methodology, and besides, economists have already expressed themselves clearly on the question, taking their cue from logicians and mathematicians. What then may be said for or against the employment of statistical measurements, and what at last analysis is our warrant for giving them greater weight than heretofore? Remembering that all knowledge is conditioned and relative, why should we differentiate between the values of experimental generalization and those of induction from relative frequency? In short, how may our beliefs regarding the relation of natural to social sciences be squared with the principles characterizing the chief methods of science?

Plan of this Book.—All of these topics here broached by way of illustrating a fundamental problem in economics cannot of course be discussed in a single volume however bulky, and much less in the sketch now offered. It is not to be expected that economics will speedily find a convenient point from which to start once more upon an upward path of dogmatic developments. If temporarily it has been yanked out of its familiar tracks,—or to change the metaphor—if just now weighty issues hang in the balance, it is self-evident that much time must elapse before orderly progress and an equilibrium of premises and principles, of aims and actions, is restored. If from a long-time standpoint periods of construction

cover generations, destructive criticism also extends over many years, if not over decades.

But though no pretense is made here to doing more than stating a problem partly in new terms, and partly of epitomizing what has already been done by others, notably in the field of doctrines, the work to be done is nevertheless sufficiently definite to admit of an outline in the rough. The first part of the book thus deals with a review of principles of economics, hence is a doctrinal Critique; while the second part turns on a consideration of methodological questions.

Or to state our plan still more specifically:

Chapter Two attempts to show in detail why sensationalism is untenable, and why the valuation problem has come to mean something new to economists.

Chapters Three to Five seek to give a critical estimate of the Utilitarian-Marginal (*i.e.*, classical or neo-classical, and marginal) analysis of price, distribution, and production respectively. But the treatment will be concise because much of this work has already been done, or because of the wish to keep distinctly in sight the larger philosophical questions involved.

Part Two begins with a restatement of the central problem, and then takes up the consideration of the principles of inference from the psychological standpoint, though with due regard also for the purely logical side. What is aimed at ultimately is an answer to the question how deduction and induction are related as *methods of science.*

Chapter Seven discusses law and causation, preparing for a later statement on the correct procedure in delimiting sciences, on the value of imputation both from a

logical and an economic viewpoint, and on the limits of induction.

The methods by which science arrives at laws and decides upon causal connections are briefly related in the next chapter, emphasis being put more upon aspects of measurement than upon canons of induction, while on the other hand statistics and reflection as distinct methods are set off against experimentation and the auxiliary adjuncts of exact science.

With this material for a beacon-light the ninth chapter proceeds to discuss the major points of an economic methodology. Scope and methods receive attention chiefly, but something is said also on the idea of statics, on the interrelation of sciences, and on ethics as a normative discipline differing from mere description.

Finally, in the tenth chapter a program of reconstruction is added to a summary statement of what economists will probably have to abandon in order to be most successful. What is to be retained, and what seems suitable for investigation both by way of rejecting catallactics and by way of approaching the problem statistically, is a subject mentioned in the last pages. All in all, then, the doctrinal and methodological questions of the day are treated with a view to positive results, but that is not to intimate that theory has not rights of its own, or that a critique must do more than point out defects and obstacles ahead.

The real question is after all: How did the existing difficulties in economics arise, and what can a definite methodology mean to men who are in search of laws rather than of individual facts pertaining to a single economic system?

PART ONE

A CRITIQUE OF ECONOMIC PRINCIPLES

CHAPTER TWO

VALUATION

Sensationalism as a Theory of Valuation.—There is no better book to help us appreciate the exact nature of the psychological premises back of Utilitarian or Marginal economics than James Mill's "Analysis of the Phenomena of the Human Mind." [1] It was not only the most complete account ever given in the English language of sensationalism, but it also exercised a lasting influence upon "classical" economists from Malthus up, and this in spite of the fact that Mill himself added but little to the knowledge of his day. James Mill, the father of John Stuart Mill, was not an innovator so much as a popularizer, a man of extensive interests and remarkable powers of presentation, who took what he found and reformulated it so as to sum up concisely the views of his age. Sensationalism, which had its inception in Hobbes and Locke, and had reached full development in Hartley and Hume, was put to new uses by Bentham, and made eminently plausible and respectable by Mill, his contemporary. In this way J. S. Mill and many others became familiar with the associationistic hedonism of the eighteenth century. In this way British Utilitarian economics served as an intermediary for passing hedonism on to Marginism. And

[1] The edition here used is that of 1869, annotated by J. S. Mill and A. Bain. Most important chapters: Vol. I, chs. 2, 3, 5-6, 10; vol. II, chs. 16-22, and 24.

precisely on these assumptions elaborately expounded by Hartley and James Mill the subjectivistic economists (the Marginists) proceeded to unfold their doctrines of value, price, and income. If not always inspired directly by this sensationalism they none the less thought in its terms, the works of Jennings and Jevons setting a good example.[1a] In England and America most, but among European continental writers too, it was understood that valuations had a feeling basis, that these feelings had their roots in ideas, that pain and pleasure were measurable quantities determining wants, and that through wants in this sense economic demand helped to determine market prices.

Now, Sensationalism divided psychology into three main parts, the first leading to the second, and this to the third. Cognition, Affection, and Volition were the principal topics for discussion, the theory of desire (which necessarily has importance for us) being a derivative of the doctrine of feeling and ideas.

Sensations were the primary facts which accounted for everything else. The experiences gathered through the five senses became ideas, subject to certain laws of association and of memory. "Our ideas spring up, or exist, in the order in which the sensations existed, of which they are the copies." [2] Not only is the order in which the more complex mental phenomena follow or accompany one another reducible, by an analysis similar in kind to the Newtonian, to a comparatively small number of laws of succession among simpler facts, connected as cause and

[1a] See for instance Jevons, W. S., "Theory of Political Economy," edit. of 1879, chs. 1-2; and Jennings, R., "Natural Elements of Political Economy," 1855, pp. 181-92.

[2] Mill, "Analysis," vol. I, p. 78.

effect, but the phenomena themselves can mostly be shown, by an analysis resembling those of chemistry, to be made up of simpler phenomena." [3]

The old notion of establishing a parallel between the facts of the physical world and those of the mind is here seen to rise to the surface again. It was one of the cherished dreams of the eighteenth century to do for social events what the preceding epoch had done for physical events; and this faith in a set of laws governing the mind and human conduct wasn't shaken until transcendentalism gave rise to a new theory of knowledge.

The key to sensationalism as a theory of knowledge lay, however, not in this monism, but in the principles of association which Aristotle had first announced, and to which the British thinkers reverted in a heroic attempt to explain knowledge "empirically." The fact that we connect events in time and space, that sequence and contiguity are common relations, and that differences or resemblances impress us from the beginning of life,—this fact was made the basis for a doctrine that by chains of reasoning we copy faithfully the relations outside. Through association impressions were welded into ideas or concepts. Through association individual ideas were built into concatenations that underlay argument and exposition, narration and description. In short, the laws of association were held to give regularity to human thought, just as the universality of certain sense impressions provided a common bond among all people. Hence, what could follow but uniformity of beliefs and a transfer of experiences through memory from one object to another? The workings of the human mind seemed explicable, once

[3] Preface, p. viii, written by editors.

the supreme principle of association was applied to both ideas and to feelings. In the words of Mill: "Not only do simple ideas, by strong association, run together and form complex ideas, but a complex idea, when the simple ideas which compose it have become so consolidated that it always appears as one, is capable of entering into combinations with other ideas, both simple and complex." Thus "brick is one complex idea, mortar is another . . .; these ideas, with ideas of position and quantity, compose my idea of a wall." [4]

Physical data somehow were transformed into psychical ones, and marvelously intricate events of consciousness appeared to have a tangible basis in senses responding to outside stimuli.[5] Feelings could therefore not be anything but sensation "sensed" in a certain way. The primary stuff was the sensation itself; the state of feeling a compound of ideas and bodily conditions, and invariably either pleasant or painful to experience. "Sensations and ideas are both feelings"; and "having a sensation and having a feeling are *not* two things." [6] Through re-arousal from within sensations originally received from without were brought back to life, so to say; Hence, whatever intensity the idea boasted at the first was transmitted in the re-arousal to the accompanying feeling. All sensations were resurrected in full strength through the agency of thought. An intellectualistic theory of feelings was thus developed by the British psychologists. Feelings, according to Th. Brown and Mill,

[4] Vol. I, p. 115.

[5] Compare this with Russell's (B.) statement: "All psychic phenomena are built up out of sensations and images alone," in his "Analysis of Mind," 1921, pp. 279 and 297.

[6] Mill, "Analysis," vol. I, pp. 224-25.

followed the laws of association exactly as ideas did. Feelings had only quantitative, not qualitative aspects. The main point was the rebirth of feeling (through memory) in its first vigor, and the reduction of all feelings to two psychic magnitudes, viz., pain and pleasure. "To have an idea and the feeling of that idea," we are told, "are not two things; the feeling and the consciousness are but two names for the same thing."[7] An idea is a feeling that "exists *after* the object of sense has ceased to be present."[8] Again: "It is easy to prove that the idea which forms part of memory is called up in the same way, and no other. . . . The idea or the sensation which preceded the memory is one of those which are calculated, according to laws of association, to call up the idea involved in that case of memory"; and it is "by the preceding idea or sensation that the idea of memory was in reality brought into the mind."[9] And again from Th. Brown, the author of "Lectures on the Philosophy of the Human Mind": "The past feelings of the mind are, as it were, objects present to the mind itself, and acquire thus truly a sort of relative existence which enables us to class the phenomena of our own spiritual being as we class the phenomena of the world without."[10] Memories of *feelings*, then, are real, and what we felt at the moment the external stimulus acted upon us we rehearse mentally, or rather feel once more upon recollection. But recollection itself operates through associations, so that feelings are associated with things not because these

[7] *Ibidem.*
[8] *Ibidem*, p. 52.
[9] *Ibidem*, p. 321.
[10] Edition of 1854, published by Masters, Smith & Co., vol. I, Lect. 9.

themselves originally aroused them, but because these things are connected retrospectively with other facts that gave rise to those feelings.

From all this it follows that desire is largely a function of associations. But of course it was also understood, for other reasons, that we desire chiefly or solely what gives us pleasure, abhorring what is painful. Hedonists from the start had no hesitancy in accepting this axiom, since it seemed reasonable that pleasurable experiences were life preserving—the exceptions being insignificant—while painful events injured the Self. On this supposition, therefore, pleasure and pain became synonyms for desire and aversion, or for life preserving and life menacing facts, or for useful and injurious things, or in fine for good and evil. Utilitarianism had its root in this rather naïve view of ethics, although it gradually qualified its defense of self-interest. J. S. Mill, as is well known, made it his duty to bring out clearly the social aspects of utility. In effect hedonism was really discarded and something else substituted, notwithstanding the retention of the older word.

People, then, desired things in proportion to their intensity of pleasantness, and shunned them according to their painfulness. Memories brought back the full quota of pain or pleasure. Wants were the proof of value in the sense of pleasurable events being "good" or at least "valuable." Hence, to want anything was to demonstrate its value. And since most sources of pleasure consisted of economic goods, Nature being chary of her gifts, wealth turned out to be a prime fountain of pleasures and wants. Abstinence, conversely, was painful; for not to consume was to miss the pleasant sensations ex-

cited by inner or outer use of commodities. Just as labor figured as a source of physical discomforts, so postponement of consumption seemed necessarily to beget painful feelings. Wants always were for the sake of sensations, and these would fail in the absence of consumption. Economics consequently had fundamental truths to work with.

To quote once more from Mill: "My state of consciousness under the sensation (as such) I call a pleasure; my state of consciousness under the idea, *i.e.*, the idea itself, I call a desire." [11] Pleasure therefore is the counterpart of desire, or vice versa. "The term 'idea of a pleasure' expresses precisely the same thing as the term 'desire.' " [12] "The idea of every pleasure associated with that of an action of ours as the cause is a motive." [13] People are actuated in that manner. Through ideas as bearers of pleasant reminiscences actions are initiated that express the desire for things, if action be needed to satisfy that longing. "The action of muscles follows, as an effect its cause, first upon sensations, secondly upon ideas." [14] To put this last thought more clearly: Sensationalism was a theory of action as well as a theory of thought. It not only gave the reasons why sense gratification would be a key to judgments of value, or to values regardless of judgments, but it likewise provided a basis for that broader policy of Non-Interference which began with Adam Smith and reached its culminating point in the abolition of the Corn-Laws in England. It was understood, from the basic data of sensationalism, that men

[11] Mill, "Analysis," vol. II, p. 191.
[12] *Ibidem.*
[13] *Ibidem,* p. 258.
[14] *Ibidem,* p. 348.

would not do what went against their own interests, and that they could not pursue ends which, in the long run, imperiled the welfare of society. Self-regard was held to be a guarantee of social conscience. An identity was found between personal and public interests. If a man was to produce to the utmost and thereby prove of value to his fellowmen, he had to be accorded freedom of conduct no less than liberty of conscience. Let a man find out when his toil caused more pain than the fruits of toil promised pleasure, and he would correspondingly shorten his work-hours. Leave to the average man his rights of asserting himself and plying his trade, as his native attitudes were sure to bid him do, and there would follow bold endeavors and a degree of efficiency which would excel all the endeavors regulated by the body politic. Thus, through a postulate of dominant acquisitiveness and maximum incentive in unrestrained contests among individuals of unequal capacities and tastes, there was reached a major conclusion on the relation of the individual to society, and of self-expression to prosperity, whose force did not spend itself for nearly a century, and whose impress upon many a treatise on economics can plainly be seen. Economics since 1800, indeed, is hardly intelligible without this grasp of the twofold root of *laissez faire* which grew out of an eighteen century soil. Sensationalism both as a theory of valuation, and as a theory of motivation for activity exercised an abiding influence upon students of social processes.

Modern View of Feelings.—If, now, we inquire into the validity of this set of doctrines we are, of course, met first of all with the well-known fact that for a long time psychologists have worked along other lines, rejecting the

bulk of associational and hedonistic teachings, and building their generalizations on facts which, if not always experimentally established, were certainly freed from the dangerous à priori assumptions that earlier thinkers deemed perfectly safe. The psychological theory of valuation to-day is quite different from what has here been presented as sensationalism. But furthermore, the question of value and of valuation has been given a larger setting, so that by the end of last century there arose a theory of axiology which, as remarked in the preceding chapter, brought to the fore many subjects of a philosophical and logical or esthetic nature. In general, the valuation process has been found to embrace so much more than was once suspected, and has been made to include so many phases of consciousness and reasoning, that it would be impossible in a few pages to do justice to it. Philosophers as much as psychologists have occupied themselves with the principal topics. Theories of logic and of epistemology have revolved about the analysis of valuation or of value. It has been concluded, largely from this angle, by one writer that "value is irreducible to such existential categories as pleasure, satisfaction, or causality." [15] A dualistic viewpoint has separated value from events or their regularities.[16] A considerable literature has dealt with the subject, but without yielding agreement on more than a fraction of the points brought up for discussion. By one authority of excellent repute

[15] Brogan, A. P., in *J. of Philos.*, 1921, pp. 197-209. See also: Moore, J. S., in same journal, 1910, pp. 282 ff. on definitions of value; Montague, W. P., 1914, pp. 353 ff.; and Perry, R. B., 1914, pp. 141 ff.

[16] See for instance Simmel, G., "Philosophie des Geldes," 2d edit., p. 4; and discussion of Voluntarism in Chapter One of *this* book.

we are told in 1915: "It is probable that psychological analysis has said very nearly all it can" [17] on the matter; but by another five years later: " . . . the study of values, far from having been completed in the existing literature, is yet in its youth." [18] Thus it would be presumptuous to examine more than a small portion of the opinion now authoritative on the essentials of valuation. But it is nevertheless possible to treat them from a purely psychological viewpoint, stating what seems most nearly secure in the eyes of psychology, and indicating in this manner the errors of sensationalism, or by implication those which will compel economists to abandon the project of correlating prices quantitatively with psychic states.

We note then first of all that, even though we identify all states of feeling with values or states of valuation—an assumption not demanded by facts,—these feelings are not for the most part of the kind that sensationalism considered. That is to say, pleasure is not the same as pleasantness, nor does a special sense of pleasure exist. There is a sensation of pain, as experiments have proven, and for it nerve endings have been located. But this is not true of pleasure, nor even of pleasantness in another meaning of the word. "Pleasant sensations," so far from being one of the most important facts of psychology, "have no existence";[19] such is the modern view; and to

[17] Urban, W. M., in *Psychological Bulletin,* 1915, p. 218.

[18] Picard, M., in *Journal of Philosophy, Psych. and Scientific Method,* 1920, pp. 11-20.

[19] Pillsbury, W. B., "Fundamentals of Psychology," 1916, pp. 449, 451. On differentiation between pleasure and pleasantness see also Moore, H. Th., "Pain and Pleasure," 1917, chs. 1-4; Young, P. T., in *Am. J. of Psych.,* vol. 32, pp. 52-3; Wohlgemuth, A., in *Brit. J. of Psych.,* 1919; Lipps, Th., in his "Psychologische Untersuchungen," 1912, vol. II, Part I, pp. 81-110. Others of similar opinion: Kuelpe, Marshall, Fite, and Miss Calkins.

this might be added the reminder that, if by pleasure states-of-pleasantness are meant, then the persistent seeking of it will inevitably defeat its own end, as experience has taught many a one to his chagrin.

But to go farther. While some difference of opinion exists [20] as to the relation of sensation to feeling, or of the latter to affection, it is granted by most that the first two are by no means the same thing. "The feelings," it is widely understood, "cannot be identified with any peripheral nervous mechanism or process," [21] which certainly is true of sensations. "Affective elements have no special physical stimuli, and in this respect resemble visceral sensation." [22] Or in the words of another authority: "Feeling is as much subjective as attention, while sensation is dependent altogether upon the physical environment. It is in this sense that feelings are subjective, sensations objective; and coupled with this subjective character of feelings is the further fact that an experience, when recalled, does not always have the same feeling as at first." [23] "Ordinarily feelings arise through excitation by some stimulus, and are closely connected in origin with sensations. But we may have both feelings and sensations from the same stimulus at the same time, and can always distinguish them." [24] "Affections are always coextensive with consciousness, diffused over all the sensory contents present at the time; and . . . if the pleasant-

[20] For identification of feelings and sensations see, e.g., Messer, A., "Empfindung und Denken," 1908, pp. 10-33. A Review of controversy is given in the *Psych. Rev.* of 1908, by Meyer, M.: "Nervous Correlate of Pleasantness and Unpleasantness."

[21] Dunlap, K., "System of Psychology," 1912, p. 244.

[22] *Ibidem*, p. 245. But see also p. 250.

[23] Pillsbury, W. B., "Essentials of Psychology," edit. of 1911, p. 260.

[24] *Ibidem.*

ness of a taste is localized in the mouth [a point used by others to identify feelings and sensations], that is simply because consciousness itself, under the experimental conditions, has been narrowed down to a taste-consciousness." [25] To be sure, feelings like sensations have duration, intensity, and quality, but only sensation has the attribute of clearness in addition, while "we cannot attend to an affection." [26] Like sensation, affection does probably follow certain laws of a quantitative relation between stimulus and response, but generally speaking it seems certain that "affection depends less upon the several and separate attributes of stimulus than upon their combination." [27] "The affection of any given moment depends upon the interplay or concurrence of sensory processes that are combined in a certain conscious pattern." [28] Thus, all things considered, sensations must be differentiated from feelings. As a recent experimenter put it: "Feeling-elements are not attributes or functions of sensations or other cognitive processes, but a separate class of conscious processes." [29]

Furthermore, so far as pleasure or pleasantness alone is concerned, the hedonistic view of a simple sensation has been replaced by a physiological one that, while equally appreciative of the survival function of pain-pleasure, takes much more account of their emotional

[25] Titchener, E. B., "Textbook on Psychology," 1910, p. 234; also §§ 69-70.
[26] *Ibidem*, p. 231.
[27] *Ibidem*, p. 259.
[28] *Ibidem*, p. 258.
[29] Wohlgemuth, A., in *Brit. J. of Psych.*, 1919, p. 210. See also Warren, H. C., "Human Psychology," 1920, p. 279; Hunter, W. S., "General Psychology," 1919, pp. 204-07; Jodl, F., "Lehrbuch der Psychologie," edit. of 1916, vol. II, p. 13.

and organic-kinesthetic accompaniments. Feelings of pleasure, so far from originating principally in sensualities, have been studied as an expression of surplus energy, as a by-product of acts of adjustment to difficulties and surroundings in general, or as a solution of tasks deliberately or involuntarily shouldered. In other words, an ideological background has been given to what once appeared to be purely physical data. "Pleasure," we are informed by one writer, "is primarily the characteristic emotional tone of affect which accompanies the successful discharge of libido along a conative channel, and the attainment of the appropriate end." [30] The whole body and being is involved in such experiences. "Conduction by units [i. e., the neural] in readiness is satisfying, while conduction by units in unreadiness and readiness without conduction, are annoying." [31] From a biological standpoint pleasure thus appears to mean feelings of harmony resulting from a successful execution of plans, from a sort of balancing of outgo and income, or from a smooth working of metabolic processes. Sensory and motor reactions are pictured as complementaries that produce pleasant states of consciousness even when they cannot be localized. "When any original behavior-series is started and operates successfully, its activities are satisfying, and the situations which they produce, also"; [32] or in the words of a European writer: Pleasure is the "proof of an unhampered psychic process

[30] Tansley, A. G., "The New Psychology and Its Relation to Life," 1920, p. 67.

[31] Thorndike, E. L., "Original Nature of Man," 1913, p. 128 (vol. I of his "Educational Psychology").

[32] *Ibidem*, p. 124.

attaining its object conformable to its natural inclinations." [33]

Such affective conditions, it has been said by some, may be remembered; and notably a French group of psychologists has treated of memory and logic as a problem in feelings.[34] The majority of students, however, have taken the opposite stand, denying that what is remembered is the feeling itself. "To have an affective memory," we are reminded by one authority, "is to be able to reinstate or recall an affective process which has once been experienced"; [35] but the prevailing opinion may perhaps be stated in this remark by an American that "if we identify feeling with one of its aspects, pleasantness—unpleasantness, and then ask whether an hedonic tone may be recalled, there is indeed none but a negative answer to that question." [36] Nor do the leading principles of association provide a theory of valuation through rearoused experiences of pleasure, for researches so far indicate that "there is no constant relation between the

[33] Nadejde, D., "Biologische Theorie der Lust und Unlust," 1908, p. 72. Similarly Marshall, H. R., "Pain, Pleasure, and Esthetics," 1894, p. 347; MacFarlane, J. M., "Causes and Course of Organic Evolution," p. 616; Moore, H. Th., "Pleasure and Pain," 1917, p. 104, and chs. 1-4; Lipps, Th., "Vom Fuehlen, Wollen und Denken," 1907, p. 243. That organic-kinesthetic expressions necessarily accompany feeling is denied by Young, P. T., in *Am. J. of Psych.*, vol. 32, No. 1, pp. 52-3.

[34] Ribot, Th., "Essai d'Imagination Créatice," 1900; also his "Psychology of the Emotions" (English transl.), 1897, Part I, chs. 11-12; Stumpf, C., "Ton-Psychologie," 1883, vol. I, pp. 1-133; Witasek, St., "Grundlinien der Psychologie," 1908, pp. 55-63, showing Freudian applications; Storring, G., "Psychologie des Menschlichen Gefühlslebens," 1916, pp. 90-1.

[35] Hunter, W. S., "General Psychology," 1919, p. 212.

[36] Urban, W. M., "Valuation," 1908, p. 114; 120-30. See also *Psych. Rev.*, 1901, pp. 262-78 and 360-70.

feeling-element of a sense-experience and the feeling-element of an associated idea." [37] What sensationalism therefore had to say on the force of association in actuating men, and adapting endeavor to transferred aversions or preferences, can really carry no weight today.

More than this, the measurement of both sensations and feelings is something quite beyond our abilities, a fact which must prove of the utmost importance, of course, in any estimate of sensationalistic value theories.

Not even the developments of psycho-physics, which by some have been supposed to make out a good case for sensationalism, are in reality favorable to it. For though it is true that relative intensities of sensations have been measured, and constant ratios of increments in stimuli to perceivable differences in response exist, these discoveries cannot avail a theory of valuation in the economic sense. Nor do they apply, of course, to feelings as distinct from sensations.

To begin with, the Weber-Fechner law does not measure the absolute magnitude of sensations so much as their relative intensities. The founders of the law, because of the nature of their task, had to select some one degree of sensation as a standard for all others, and for their purpose made the least just noticeable intensity a zero point in their scale, calling it the *limen*. Interest thereafter turned on proportions of stimuli and response, and on the relation between additions to stimulus and just noticeable increases of response. It was found, for example, that "a stimulus must be increased by a certain constant ratio in order that the sensation might be just

[37] Wohlgemuth, A., in *Brit. J. of Psych.*, 1919, p. 238.

noticeably more intense each time." [38] In the second place, however—ignoring the limited field within which the law rules, and certain irregularities in the law of relative intensities [39]—we have to face the important fact that almost from the outset differences of opinion arose as to the nature of the Weber-Fechner law.

To the authors themselves it was a psycho-physical law which in part was to do for psychology what the laws of mechanics did for physics. Yet this is not the interpretation commonly accepted to-day, nor do the later views support in any way the contentions of sensationalism. For if we follow the physiological argument (which, incidentally, seems to have the better of it) the measurement of sensations becomes a certainty. But then modern theories of valuation and our personal experiences of valuation are not affected, since value is not a matter of sensation. To grant then that "the facts available indicate that the law [of Weber] is due to the increasing resistance offered in the nervous system to the transmission of the more intense nerve impulses, and that the explanation is physiological rather than psychological or purely psychological" [40] is not to throw light on economic values. On the other hand, if we stand by Wundt and others, convinced that "Weber's Law cannot be deduced either from the physiological peculiarities of the nervous substance, or from a functional relationship

[38] Hunter, W. S., "General Psychology," p. 265. But see also Dunlap, K., "System of Psychology," 1912, pp. 111-12. For Fechner's own statement see his "Elemente der Psycho-Physik," 1860, 3d edit., ch. 9 of vol. I. See also Titchener, E. B., "Textbook of Psychology," §§ 66-67.

[39] Fechner, Th., "Revision der Hauptprobleme der Psycho-Physik," 1882; Warren, H. C., "Human Psychology," p. 219.

[40] Pillsbury, W. B., "Fundamental of Psychology," 1916, p. 215; Mueller, G. E., "Grundlegung der Psycho-Physik," 1878.

between the physical and the psychical; for it is founded in the psychical processes which are at work in the comparison of sensations. It is in this sense not a law of sensations, but a law of apperception" [41]—if we allow this, we deny the measurability of sensations, thus leaving one of the main assertions of the hedonistic school unproven. In either case economists must turn to new constructions on the psychological side.

In short, the affective aspects of valuation do not provide nearly so simple a solution of the valuation problem as had once been thought. Pain is not an opposite of pleasure, and the real opposites of pleasantness and unpleasantness are not the simple units that the eighteenth century spoke of. We cannot treat them as integers that submit to addition and subtraction.[42] There is evidence, even, that the two are not mutually exclusive, but may coexist as factors of different, incomparable qualities.[43] And as for *feelings*, as distinguished from sensations, we have none of the data which Weber and Fechner succeeded in building into a physiological law. What experiments have been conducted, have aimed at indirect measurement by correlating affective states with physiological changes. But the correlations have not so far been very satisfactory.[44] We are told, regarding these experiments, that "the feelings show marked bodily accompaniments, but these cannot be said to correspond

[41] Klemm, O., "History of Psychology" (transl. by Wilm, E. C., & Pintner, R., 1914), p. 263.

[42] Jodl, F., "Lehrbuch der Psychologie," 4th edit., vol. II, pp. 8-9; Lipps, Th., "Leitfaden der Psychologie," 1906, Part Seven.

[43] *Ibidem.* Also: Wohlgemuth, A., in *Brit. J. of Psych.*, 1919, p. 239; Ebbinghaus, Sully, and McDougall, W., in his "Introduction to Social Psychology," p. 156.

[44] Jodl, F., "Lehrbuch der Psychologie," 1916, vol. II, pp. 27-8.

accurately to the differences between pleasantness and unpleasantness, although the *degree* of our feeling carries with it an approximately corresponding amount or intensity in the accompanying physical expression." [45]

Cognitive Aspects of Value.—So far as the principal modern notions on sensation and feeling are concerned, therefore, it would matter little for the economist whether he leaned toward a feeling or an intellectual view of values. It is indeed almost self-evident that values of many kinds exist and that, according to our definition of the term, either affective or cognitive phases may be stressed. The history of theories of valuation shows this instructively. There have been those—particularly at the beginning—who have sought the key to it in feelings or volitional attitudes arising from feelings, just as the sensationalists reduced all appetites to memories of sensation.[46] But later on the analysis of value has turned chiefly on the complexity of the problem, on the necessity of subdividing the value class, of acknowledging the difference between valuation in its functional aspects, and on valuation as a formal judgment.[47] Yet for the economist

[45] Pillsbury, W. B., "Fundamentals," p. 460.

[46] Meinong, A., in "Psychologisch-Ethische Untersuchungen zur Wert-Theorie," 1894, but this view was modified a little later (see below). Also: Jodl, Kreibig, Simmel, and Haering. Writers stressing the volitional side are: Eisler, Muensterberg, Frischeisen-Koehler, Wundt, Lipps, and Brentano, but especially also: Ehrenfels, Chr. v., "Von der Wert-Definition zum Motivationsgesetze," 1896 (pp. 103-22), being vol. II of his "System der Wert-Theorie," 1897. Partly in agreement with him: Meinong, A., in *Archiv f. Systematische Philosophie,* 1895, pp. 327-46, and in his "Annahmen," 1902. See also Perry, R. B., in *Quarterly J. of Ec.,* 1916, p. 449. The best one volume treatise in English is probably Urban, W. M., "Valuation," 1908, dealing with all aspects of the problem. Others: Kraus, O., "Zur Theorie des Wertes," 1901, ch. 7, and article in *Jahrb. der Philosophie,* 1914; Picard, M., in *J. of Phil., Psych., and Scientific Method,* 1920, pp. 16-17.

[47] E.g., Urban, W. M., "Valuation."

this choice in itself is of no moment. For if he does resolve values into affective states, he is still confronted with the fact that these latter differ *toto coelo* from the hedonic tone familiar to sensationalists. To trace all valuations back to feelings—supposing it seemed necessary—would consequently not mean a substantiation of the psychology back of Utilitarian or Marginal economics.

There are however two points that need emphasizing in the treatment of values as cognitive processes; and inasmuch as these judgment-values play a prominent rôle especially in economic life, our emphasis can hardly be too great. Namely, in the first place, perceptions and ideas are never built directly out of sensations, as sensationalism believed, and in the second place value-judgments and feelings have most frequently a social basis, so that a strictly individualistic hedonism could not explain them. Valuations from the apperceptive standpoint, in short, are just as far from being what eighteenth century writers asserted as they are according to current views on the feelings and emotions.

The nature of perception, ideas, and concepts is a commonplace in modern psychological texts, but the special purpose to which we put this knowledge will justify a few quotations. The hiatus from physiological to psychological facts which they make clear must be deemed fatal to all sensationalism, whether adapted to epistemology or to axiology.[48]

We read in one text: "It is evident that the object seen depends not only upon the sensations that affect the

[48] For philosophical phases see Chapter Two of *this* book, section on German Voluntarism.

sense-organ, but also upon the memories that one possesses, and the laws of association and recall." [49] "The facts of perception—stand in quite different relations to one another from the physical facts which stimulate us through the eye and other organs." [50] The rise of an idea, thus, might be explained somewhat as follows according to the same writer: "A sensory stimulus sends a nerve impulse to the brain. At some synapse in its central course, part of the impulse is distributed from the main path into an adjacent neuron. This overflow current, being less intense, loses its own mode and takes on the characteristic mode of the neuron into which it passes, this mode being determined by the trace left by past stimulation. The resulting central process is not a sensation, but an idea; it no longer retains the characteristic of its own origin." [51] This is the dominant view of perception and ideas, even though on minor points disagreements exist. Thus there are those who deny that ideas are "centrally excited sensations;" [52] but even then it is granted that "the perception of an object and the proper adjustment to it depend not so much on what is directly present in the focus of consciousness, but on the wealth of accumulated material lying outside the moment-focus." [53] "The image, representation, or idea of a table," from this standpoint, "is not itself a table; nor is it a synthesized sensory compound referring to the object, table; it is a psychic element referring to the sen-

[49] Pillsbury, W. B., "Fundamentals," p. 160.
[50] Warren, H. C., "Human Psychology," 1920, pp. 12, 66, 88.
[51] *Ibidem*, p. 226. See also Stout, G. F., "Analytical Psychology," vol. II, p. 7; Titchener, E. B., "Textbook of Psychology," p. 48.
[52] Sidis, B., "Foundations of Normal and Abnormal Psychology," 1914, p. 138.
[53] *Ibidem*, p. 258.

sory compound on its objective aspect." [54] In any case, perception and ideas have no definite, or fixed, quantitative relation to the elements in sensation out of which they were, according to sensationalism, directly constructed. What psychologists generally emphasize is the complex nature of percepts and images, or the derivation of perception from four facts, viz., a present sensation, present relations bound up with the existing sensation, an imaginal content, and certain emotional adjuncts (in many cases).

Concepts, also largely for this reason, are something very different from the mechanical constructs that associational psychology believed; for they are really "an imaginative content in which the relations are the central feature, and the sensory factors purely incidental." [55] In concepts relations are the principal theme. "Conception is the relational consciousness—of a group, or of an object as member of a group." [56] Thought thus is "consciousness of objects not actually stimulating the special sense organs through which they were primarily perceived," while images are "objects-thought-of." [57] Or to use the phrase of another authority: Thought is distinguishable from other mental states, in that its "ideational components are symbolic," and its "development is due almost wholly to the social environment." [58]

This last statement points to a further fact about ideas in general, and more especially about value-judg-

[54] *Ibidem*, p. 366.
[55] Dunlap, K., "System of Psychology," pp. 166-67.
[56] Calkins, M. W., "First Book in Psychology," 4th edit., pp. 146-47.
[57] Dunlap, K., "Mysticism, Freudianism, and Scientific Psychology," 1920, p. 140.
[58] Warren, H. C., "Human Psychology," p. 314.

ments, namely their social origins in one aspect. Individual psychology deals with one side of cognition and trains of thought or of feelings, but sociologists have the right to study *inter-individual* relations, in the light of which our personal notions assume a broader significance. Thus it goes without saying that little of what we know is, strictly speaking, self-earned, and that everything we believe or do is influenced by the thoughts and actions of our contemporaries. The majority follows, and a small minority leads. Innate differences and those which life's experiences and an objective physical environment bring out, or accentuate, lead to standardization of creed and conduct. All human beings are, by inborn predisposition, as fond of subjecting themselves to others, as of asserting themselves. Suggestibility is a common heritage for men in all ages. Imitation plays its part in uniformizing thoughts and purposes. While on the one hand our congenital and acquired differences and peculiarities prevent a dead level of social expression, on the other they also make possible that degree of organization which, if not the same as agreement, none the less tends to perpetuate conformity. Through natural or artificial means of communication, through coöperation in many fields, including the economic, and through control exercised by virtue of the differentials just mentioned, norms are established that obtain over wide regions, swaying many millions of people. Personality and genius, technical expertness of professions and "trades," and government in various guises—all this makes for standardization. As the spokes of a wheel point from a center outward toward the rim, so the influence of the élite, that is of leaders in politics, religion, art, science, and in-

dustry radiates out, establishing contact with the millions who look for precept and example. All sorts of values are defined by a minority, communicated to the masses, and put to the test thereafter. The average person gives advice of his own, and in a measure contributes toward the norms and practices of his age; but the greater credit must go to those who excel beyond comparison, and thus give much more than they can possibly take. Values owe their definiteness and permanency to this circumstance. What habit is to the individual, that custom is for society. We learn from infancy by listening to elders, by copying the deeds of others, by assimilating rather than by inventing new means or ends. Through social heredity our slender inborn resources are made enormously productive and profitable to ourselves. We do more by rote than by reflection. Just as movements of the body become automatic through continued exercise, so our ideas and evaluations become stereotyped, a reflex of thought elsewhere originated, and proof of the power of office, organization, ritual, and personality over the destinies of the mediocre.

Laws of learning and of sociation therefore compel us to recognize the plural sources of value, even though we individualize necessarily its emotions. All values, the economic not excluded, have an impersonal aspect, and students in several fields have rendered us a signal service in describing these principles. Particularly during recent decades the sociological phases of value have been clearly presented. Economic values presuppose others of a more fundamental mission. This has become evident. Absolute or non-exchange values rule logically prior to market prices, even though most non-economic norms lend

themselves to a pecuniary scale of measurement.[59] Value "as an objective social fact is the product of social interaction . . . , but as a product of social interaction it is the resultant of modifications of the subjective feelings of value of individuals . . ." [60] "Economic value is a function of interacting and reacting minds." [61] "No analysis of a valuation ever gives us a complex in which values are not presupposed." [62] "Value is an expression of organization definite in proportion as it is institutionalized." [63] "The progress of market valuation, as a rule, is a translation into pecuniary terms of values which have already become in some measure a social institution . . ." [64] "Both in legal and in economic values we have an elaborate and complex system of social psychological character, which can by no means be reduced to elementary desires or feelings, even though . . . no part of the system will be found outside the minds of individual men." [65]

Value in its Volitional Aspect.—It follows from this social origin of valuations, which cannot be quantitatively related to individual sensations or even to percepts, that in their volitional aspects also they are something very different from what sensationalists had taught. As a theory of motivation also the hedonistic psychology

[59] See for instance Veblen, Th., "Economics of Enterprise," 1904; Simmel, G., "Die Philosophie des Geldes," 2d edit., 1907, ch. 5, pp. 387-479.

[60] Urban, W. M., "Valuation," p. 317.

[61] Perry, R. B., in *Quarterly J. of Ec.*, 1915-16, p. 475.

[62] Haering, Th., "Untersuchungen zur Psychologie der Wertung," quoted by Urban, in *Psych. Bulletin*, 1915, p. 219.

[63] Cooley, Ch. H., in *Quarterly J. of Ec.*, 1915, p. 9.

[64] Anderson, B. M., "Value of Money," p. 30. See also his "Social Value," 1911; and Ehrenfels, Ch. v., "System der Wert-Theorie," vol. I, p. 170.

[65] Reference mislaid.

is untenable to-day, not merely because our prevailing treatments of ethics are hostile to it, but in the more serious sense that our notions of human nature, of instincts and the emotions, of the modifiability of the instincts, and of the general law of progress and moral developments have little in common with the earlier philosophy. Partly because of our dissent from the analysis of feeling and cognition current among the founders of economics, and partly owing to our more accurate understanding of social processes we are obliged to reject Bentham's theory of motivation which was derived so logically from his theory of values.

One of the divisions into which a theory of motivation naturally falls need not, however, concern us here. What in a complete treatment would have to be said on the nature of the Ultimate Good and on the principles of sociation thanks to which one public policy rather than another must be best, may here well be omitted, for it is not the ethical system of Utilitarianism that matters in an appraisal of theories of value, but the question whether men are indeed actuated as sensationalism told us, and whether values necessarily and invariably have an individual aim, putting the Ego always in the center and promoting social welfare precisely because of this fact. Naturalism, to be sure, did not take this view of motivation and morality, nor was it even accepted by all of the British Utilitarians, to say nothing of continental writers. But generally speaking the hedonistic theory of motivation, which pictured society as a mechanical aggregate of individuals, did ignore values that the instinct of self-interest, catering directly to the self, could not explain. All wants being values (though not all

values need be wants), they were traced back to pleasant experiences or to ideas of them, sensation being the ultimate fountain of all.

Now, in the light of modern knowledge a line must first be drawn between what is innate and what is acquired after birth in human motives, for both manifestly play a rôle in history. Because of it man has a record of evolution or development radically different from that of other species.

As for the instincts, opinion is no longer as unanimous as some twenty or thirty years earlier, particularly since geneticists have forced us to inquire into the nature of those carriers of heredity which make human nature fairly constant. If until recently instincts were enumerated and treated as perfectly definite facts, nowadays the attitude of many is somewhat skeptical. What is instinctive and what is not, has once more become a vexing question. We read for instance in an article of very recent date: "An instinct, since it is as much a unit character as any other product of Mendelian inheritance, is inconceivable apart from the fact of its structure." [66] The emotions which, thanks mainly to British psychologists,[67] were taken as an index of the character of an instinct, have come in for their share of criticism, with the result that much work appears to be before us if we wish to satisfy new researches in biology. "The assumption," we are told, "of an original and unchanging characteristic central emotion, which is the essential attribute

[66] Bernard, L. L., *Psych. Rev.*, 1921, p. 103, 109, 117. Also Tansley, A. G., "New Psychology," Part II; Faris, E., *Am. J. of Soc.*, 1921, pp. 184-96.

[67] McDougall, W., "Introduction to Social Psychology"; Shand, A. F., "Foundations of Character," 1914; Marshall, H. R., "Pain, Pleasure, and Esthetics," 1894, pp. 83-86.

of the instinct, is itself without foundation in the data." [68] However, in general instincts may still be said to promote self-preservation, to be inborn and not acquired, to be quite plastic as a structure or process, subject to all kinds of postnatal experience, to lack a reasoning element, to be accompanied by feelings, and to center as a rule upon a near-by object. Instincts thus have no ideational basis, nor is a motive or an act of deliberate valuation a part of an instinctive reaction.[69] So much we are tolerably sure of. Within this range of facts instincts have proven on the whole selfish, albeit their usefulness from a phylogenetic standpoint is not, of course, thereby questioned.

Not all wants are, however, instinctive. Conative processes are one type of will or wish or routine activity on the part of individuals, but not the whole of it. Rather, ideas also lead to volition, and in the opinion of many constitute a prime characteristic of most acts of will or states of longing. Thus, what needs to be stressed is the reality of the modification of instinctive predispositions, as well as the possibility of men acting irrationally not only when driven by instinct, but likewise when translating ideas into motor-reactions.[70] Stimuli, in other words, that animals do not encounter or cannot perceive in a psychological sense, may come from the outside.

[68] Bernard, L. L., *Psych. Rev.*, 1921, p. 105. Other writers: Kantor, J. R., *Psych. Rev.*, 1921, pp. 138-9; Calkins, M. W., "First Book in Psychology," 4th edit., 185-86, where non-economic emotions are discussed; Link, H. C., *Am. J. of Psychology*, Jan. 1922.

[69] Innate and acquired volitions are contrasted by Meumann, E., in his "Intelligenz und Wille," 1908, pp. 202-8. The independence of instincts of pain-pleasure is emphasized by Drever, Jas., in his "Instinct in Man," 1917.

[70] Importance of modified instincts for social science is shown by Hunter, W. S., in *Psych. Rev.*, July, 1920.

Men have added a cultural, non-physical, environment to that provided by nature. They live in a socio-economic world even more than amidst conditions facing the savage; and because of this fact they have also burdened —or enriched themselves, according to view—with considerations unknown to animals. To the love of offspring, gregariousness, and to weak creative instincts have been added values that made organization of the most intricate sort possible only by limiting that freedom which not so very long ago was thought to be the principal blessing of a state of nature. Mental growth increasingly has dwarfed physical strength. Concepts have given purpose to perception and imagination, and in memory-associations provided a means for adapting the potentialities of nature to non-biological needs, to wants ever multiplying and continually digressing from the line along which they originally moved. Thus ideas rather than instincts are responsible for most of our economic activities. Until direction is given to the former, the latter are not truly social or economic. Emotions still color our experiences and leave the most lasting impressions, but cortex and association areas add their part to the result which we call motivated activity.

"Desire," as one notable book puts it, "is a very complex emotional system which includes actually or potentially the six prospective emotions of hope, anxiety, disappointment, despondency, confidence, and despair." But "the prospective emotions of desire are only aroused by thoughts; being first dependent on the thought of the end, and secondly on some modification of this thought which operates as the special stimulus of one or other

of these emotions."[71] "As a rule the projection [picturing an impending act as already completed] comes first, is then felt as a motive, and then leads to action."[72] In a sense, then, *assumptions* (Annahmen) form the background of many of our desires or acts of volition.[73] We are under the sway of ideas not always pushed into the foreground of consciousness, but nevertheless partly determinative of the mode and direction of our efforts. Will consequently involves the "consciousness of an act to be performed, of the end or consequences of an act, and of an accepted purpose or intention."[74] It is with a set of beliefs and concepts that we start out, no matter how original the form of our achievements. Generally speaking we may also agree that the object of our desire is not within our present reach, and that obstacles will, under suitable conditions, intensify our striving. Or in the words of one writer: "Conative factors attach only to imaged, ideated and conceived content. If an object is present to sense—it may be pleasing or displeasing, but cannot be desired or be repugnant."[75] But whether this be so or not—and some have rejected this notion—there can be no doubt that wish and effort originate in ideation as well as in instincts.

[71] Shand, A. F., "Foundations of Character," pp. 463-4; Kenagy, H. G., in *Psych. Rev.*, 1917, p. 380. Desires traced to feelings: Jodl, F., "Lehrbuch der Psychologie," 1916, vol. II, p. 68.

[72] Witasek, St., "Grundlinien der Psychologie," 1908, p. 360.

[73] *Ibidem*, p. 351; Urban, W. M., "Valuation," pp. 38-39; Meinong, A., "Ueber Annahmen," 1902; Shand, A. F., "Foundations of Character," p. 518.

[74] Breese, B. B., "Psychology," p. 402.

[75] Dunlap, K., "System of Psychology," p. 251; Ehrenfels, ch. v., "System der Werttheorie," vol. I. For obstruction view of will see Shand, "Foundations," pp. 461 and 519; Boodin, J. E., in *Am. J. of Soc.*, 1915-16, p. 65; Ward, Jas., "Psychological Principles," 1920, p. 283; and Simmel, G., "Philosophie des Geldes," 2d edit., pp. 12-13.

From this follow two facts of significance for an appraisal of sensationalism, viz., first, that socially-derived values may supersede the purely individualistic, besides making judgment habitual, and secondly, that egoistic standards may be supplemented by altruistic ones which among lower types of animal life exist in only one form, viz. the parental instinct. Put differently, valuations may not be rational at all from an hedonistic viewpoint, and means may become ends to the effect that pain is freely courted as its own reward, or as a sacrifice whose joys, all things considered, exceed the unpleasantness experienced. Just as valuations in general are standardized through the agencies of social control, so norms of action and desire may be imposed upon the individual that his original nature might not agree with. Habituation on the one hand, and custom or control on the other hand, give rise to wants that are absolutely at variance with the end proposed by an hedonistic calculus. Neither measurements of safety and maximum enjoyment, nor the dictates of self-preservation, enter into a large number of our everyday desires. Not only is it true for psychological reasons that desire "has no definite or constant relation to the amount of pleasure that may result from its satisfaction," [76] but more especially to the sociologist must it seem self-evident that "the degree of value [of anything] varies independently of hedonic intensity." [77] From one standpoint it may seem as if "every act of conation or will, as soon as it takes effect, furthers the state of happiness as compared to that frame of mind which would emerge if the respective action had

[76] Ward, Jas., "Psychological Principles," p. 283.
[77] Urban, p. 74.

not been taken,"[78] but the question here is the *object* of our happiness, the way it affects our fellowmen, and the thoughts that urge us to do what produces pleasure in us. Viewed in this light the search for pleasure may mean nothing worse than a love of other men's approval, or the feeling of satisfaction that follows a deed of mercy. Valuations as wants then are altruistic even when apparently hedonistic. Value, more than ever, ceases to be a "single moment of enjoyment for its own sake," becoming instead "a fact separated by the judging individual from the contents or cause of enjoyment, and something desirable which presupposes the mastery of obstacles, if not of distances in time or space."[79] Or in the phrase of an American psychologist: Pain and pleasure "are ideal constructs which as objects, as passive states—are—the products of a process of abstraction exercised upon our 'condition' worths, including the primary 'condition' worths, together with their complementary values ethical and esthetic, which arise on that level."[80] Primeval values give way to secondary and tertiary values which, as want or enterprise, resemble in no wise the motives postulated by an unmitigated individualism.[81]

[78] Ehrenfels, Ch., "System der Werttheorie," pp. 32, 41, 249; Roback, A. A., in *Psych. Rev.* publications, 1918, No. 111, p. 37.

[79] Simmel, G., "Philosophie des Geldes," pp. 12-13; Urban, "Foundations," p. 86; Barrett, E. B., "Motive-Force and Motivation Tracks," 1911, p. 179; Hayes, E. C., "Sociology as Ethics," 1921, ch. 7; Taussig, F. W., "Inventors and Money-Makers," 1915, pp. 76-79; Tiburtius, J., in Conrad's *Jahrb.*, 1914, pp. 721-89; and Sombart, W., in his "Quintessence of Capitalism," Book II, p. 171 (transl. by Epstein, G., 1915).

[80] Urban, "Valuation," p. 417.

[81] On possible desire for pain-experiences see Meinong, A., "Ueber Annahmen," 1902, ch. 9, § 51; and Green, Th. H., "Prolegomena to Ethics," edit. 1906, Book III, ch. 1.

Some Conclusions.—But this being so, can we expect wants to be measurable any more than feelings or judgments or ideas? The question answers itself. It follows from all that psychologists have said on the subject of valuation that intensities of wish, or wants such as the economist is interested in, are not ascertainable by any known methods. We can be certain that wants differ in intensity, each man comparing his own with that of the next, and coming somehow to the conviction that his desires exceed, or fall below, those of the next man; but this is not measuring them as the science of economics or of psychology understands the term. Exchange values, therefore, must either be interpreted as something generically distinct from subjective valuations of any kind, including our moral and esthetic aspirations, or we must arbitrarily make a given price the index for a definite degree of want or pleasure, resorting frankly to a *petitio principii*. On the whole, then, it seems best to divorce the psychology of valuation from price, though encouraging a qualitative analysis. For practical purposes the two are incomparable, so far as our present knowledge of them is concerned. Value, we may agree, "is not determined by the particular exchange-ratio in which it happens to be put, and is not changed *eo ipso* every time a new comparison is made." [82] Valuations and wants are too elusive, too complex, and too individual, in spite of outer limits set by prevailing social norms, to be useful for a science of catallactics. Exchange itself, as has been truthfully said, "is a sociological phenomenon sui generis, a singular form and function of inter-individual life that can never be deduced logically from the quali-

[82] Anderson, B. M., "Social Value," p. 24.

tative and quantitative facts called utility or scarcity." [83] But because wants are so mobile and so far removed from mere physiological events, we are unable also to apply to them laws of response or of fatigue. To register increments of satiety in a physical sense may be possible, but to subject valuation processes to the same tests would be folly. "Since the value-feelings accompanying successive increments of wealth are judgment-feelings, while in consumption the feelings are sensation-feelings, the presuppositions being different, the law of their modification may be different" [84] also. It will remain true always, as economists continually and to good effect point out, that we have limited capacities for enjoyment and grow tired of things either while consuming them, or as possessors of them in excess quantity. But to the questions, what *is* excess, where *is* the margin of want, and what the precise degree of want at that margin, our psychologists offer no answer. Nor do they encourage us to make utility synonymous with desire or with its emotional accompaniments.

So, what is to become of our law of diminishing utility? Plainly, however suggestive our bids in the open market may be of trading motives, they cannot be explained merely by the magic word "utility." All valuations are something categorically different from physiological facts, or from exchange-rates." Adventitious values [85]

[83] Simmel, G., "Philosophie des Geldes," p. 59. See also Ehrenfels, "System der Werttheorie," 1897, vol. I, p. 93.

[84] Urban, "Valuation," pp. 164, 169: "The law of satiety does not apply to the feeling-power of value"; p. 152(-5): "The threshold of value [marginal]—has—a cognitive character which distinguishes it from the merely hedonic threshold"; see also pp. 172, 186-88. Kreibig, J. G., and Meinong, A., similarly.

[85] Watkins, G. P., "Welfare as an Economic Quantity," 1914, chs. 13-15, and 18.

lurk everywhere, and primary gratifications no longer hold the field. So a consideration of absolute values cannot rest on physiology. Nor can it be more than a preliminary in a price analysis, the main task being a quantitative correlation,

CHAPTER THREE

PRICE

Non-Psychological Premises of Economics.—Although a rejection of sensationalism as a theory of valuation must have serious consequences for systems which are bound up indissolubly with it, Utilitarian and Marginal economics might nevertheless be considered vindicated, provided nothing else were proven wrong than this reduction of want and value to sensations or feelings. It is necessary therefore, if our critique of doctrinal economics is to be thoroughgoing, to test its treatment of prices independent of all psychology, or at any rate with reference to other points than those of a particular psychology. What we must ask is: Can the traditional reasoning anent price (respectively shares of income) hold itself, supposing sensationalism were quite ignored? Or are there errors that condemn it on other grounds? Are the laws proclaimed, for instance, real laws? And in what sense has the search for them yielded results at all comparable with what the canons of science in general demand?

Now, in approaching the problem from this angle we are driven to the necessity, first of all, of defining certain terms frequently used in economics, and secondly to restate some of the premises other than the psychological, without which catallactics could not have presumed to accomplish what apparently it did.

As regards the word "law," then, we should at the outset emphasize that strictly speaking, it always means a statement of things or events regularly recurring together in time or space, subject only to such conditions as may be brought logically in harmony with said law. That is to say, the law is an abstraction treating of qualities or quantities recognized as sequences or coexistences. Our senses will present them as parts or centers of a large complex of data, but after these latter have been allowed for, the remainder is true to the law in all cases. Or to put the matter differently again: A law correlates things or events, and we "determine" one fact or set of facts by referring to those others which invariably are an accompaniment of it. If I speak therefore of "determining" the price of an article, I mean that certain things precede in point of time, or go with, that price, these regular recurrences constituting a law of price. I may bring other prices in connection with the particular one examined, or I may look for facts that are not themselves prices, such as supply or states of mind—supposing I could ascertain them—or any number of things discoverable by my method. In all instances, however, the determination of a price, whether expressive of a rigid law or not, will signify this linking of a series of facts with it. Nothing else can mean determining a price, and this it is very important to remember.

On the other hand, the words "fixing" or "measuring" a price have a less distinctive place in economics, albeit occasionally responsible for serious mistakes. To "fix" a price can mean no more than to state it, and what a price *is* we shall see in a moment. Measuring a price, as

against fixing it, must then mean that we compare quantities of different things that are exchanged for a constant amount of some other article serving as a standard. In the comparison of these, presumably different, physical amounts of different kinds of things with some one specified commodity I bring about a measurement, precisely as I may measure the length of a table and a sofa by applying a physical standard such as a meter.

This is the only possible way of measuring prices because a *price is itself the amount of one article given for another.*[1] I may think of both articles as physical quantities—if they are not services rendered—or I may consider them as values in the absolute sense. That does not matter. But invariably a price is the quantity of one thing tangible or intangible exchanged for another, and since exchange in modern times is carried on chiefly by means of money, which also acts as a standard of value, we describe a price usually as the amount of money given for an article or a service.

How much money is paid more or less regularly for a good, that is one of the chief questions with which economists have concerned themselves, and it is in watching the analysis back of these attempts at the discovery of laws of price that our attention is called to their strongly hypothetical nature. For to begin with, economics is not really intent upon explaining any one price such as businessmen make their daily study. Not particular actual prices, but rates of exchange relative to *selected conditions* are the subject of the professional student. Economists so far have always treated prices

[1] A list of definitions of price since 1769 is given by Fetter, F. A., in *Am. Econ. Rev.*, vol. 2, pp. 783-813.

as points on demand and supply curves, the factors determining that point being itemized previously and given an imaginary quantitative relation. *Premises*, in other words, have always been essential to the orthodox statement of laws of price, and these must be carefully declared, if the conditional character of economics is to be fully understood.

Apart from the theory of sensationalism, economics has worked with assumptions of private property, of freedom of contract, of freedom of vocation and residence, and with several others more or less clearly implied and to be mentioned in a moment.

Now, the substantial accuracy of the assumption of private property and freedom of contract and the other two items may be readily granted. Though restrictions by government have long been definitely made and indeed added to during the last few generations, a sufficient amount of individualism has remained to serve for the ends of the economic argument. With regard to competition, however, it must be stressed at once that it can hardly be mentioned as a premise distinct from the others. Competition, it will be seen at a second glance, is not something different from freedom of contract, hedonism, and the rights of vocation or residence, but a term rather by which we describe the aggregate effect or the psychological aspects of such legal rights and human traits. A little thought for the way in which economists have always portrayed this competitive system will reassure us on this point, and incidentally also show its relation to contract and monopoly.

As economic literature proves,[2] competition chiefly

[2] For an illuminating recent discussion of competition as a prerequisite to economic arguments see, e.g., Amonn, A., "Objekt und

meant a struggle among contestants for pleasure and gain. Hedonism itself declared men to be moved by considerations of advantage, the aim being to avoid pain and to seek pleasure. Enterprise pivoted on these two arch-dispositions innate in all humans. But men were by birth unequal. They had different endowments of strength, aptitude, temperament, and so on. They furthermore lived amidst different environments, receiving unequal training, being helped or hindered by unequal socio-economic factors before they entered the arena for gain, or while battling within it. Thus men had different chances in the game, and freedom of contract permitted these differences to make themselves felt—up to the point where other considerations might call a halt. Men were put on an unequal footing so that natural and acquired disparities might secure victory for some, and defeat for others. This is one of the meanings of competition and freedom of contract, and so a competitive régime is nothing distinct from the data examined. If men were ever so unequal, but restrained by law, their relative standing would be standardized for practical purposes. On the other hand, if contract were completely rid of regulation, the results need not be what now we associate with competition, provided congenital equality led to socio-economic equality. Or again, if competition meant simply a spirit of emulation that spurred men to action and maximum output, without any thought for proportionate reward, freedom of contract might be reconcilable with utmost control of bargaining for the acquisition and exchange of wealth. But precisely because

Grundbegriffe der National-oekonomie," 1912. For a rejection of competition see, among others, Hobson, J. A., "Economics of Distribution."

hedonism referred to acquisitive leanings, in full view of differences for work, competition had to mean a system in which differentials of nature and of personal endowment and opportunity are legalized, the equality of all consisting of their like rights under law to do the best they could, taking their fate stoically, and trying again if they failed the first time. Thus competition is a play of differentials consonant to legal and hedonistic premises, but not anything separate from them. The joint operation of the premises *is* competition.

It follows, then, that monopoly could not well mean a differential advantage only, although this interpretation has found vogue. In a sense all superiorities are, to be sure, a monopoly. At least they tend to favor the emergence of quasi-monopolies for a certain length of time and locality. But strictly speaking this is not what economists could think of, for that would have involved the repudiation of competition. Nothing of the latter would then have remained. Hence monopoly was not incorrectly defined, by some, as the ability to augment total net profits by reducing production or sales, or as the situation in which one buyer faces many sellers, and vice versa. Monopoly thus became exceptional, and competition the rule.

So far, so good. After Ricardianism had gained a hold, however, a further premise hove into view, and still another might have been added for the sake of logical consistency. Namely, in the first place, statics was conceived as a constancy of socio-economic conditions due to which abstractions along the lines already mentioned would become fruitful, yielding exact laws such as physicists could pride themselves upon. Statics was thought

to signify (quoting from a representative authority in this respect): "If there is no change in the mode of action, there is none of that grander progressive movement by which the structure of society is altered. If no labor and capital shifts its place from group to group in the industrial system, there is none of that type of movement which, in a special and higher sense, we here term dynamic. Till the ground forever with the same tools and get the same kinds of crop, work in the same mills with the same machines and materials—in short, change nothing in the mode of creating wealth—and you have a socially static industry. The producing organism then keeps its form intact." [8] Some abstraction like this was deemed to be a logical prerequisite to a clean-cut analysis of the pricing process, so that for economists statics and catallactics became virtually synonyms.

Secondly, if money served as a medium of exchange and as a standard of value it was necessary to the Utilitarian-Marginal argument that the price-level be assumed constant, and hence the amount of standard metal according to the quantity-theory of money. Such a premise was not, to be sure, specified among the others here discussed, but it might have been, since the dispersion of prices due to changes in the volume of a circulating medium or in its rate of turn-over was a familiar fact by the beginning of the nineteenth century.

Definitions.—Turning now from the presuppositions in psychology and political law to the law of price itself, we must first be careful to understand the terms demand and supply, buyers and sellers. Their definition becomes the

[8] Clark, J. B., "Distribution of Wealth," 1899, p. 59. See also ch. 3.

more important since economics, like geometry, developed many theorems from a few basic postulates.

As to demand it was always meant to refer to a want accompanied by purchasing-power, and not simply to a state of mind such as a boy's who looks longingly through the show-window, but has nothing with which to reënforce his desire. Furthermore it was generally, though not perhaps by all,[4] believed that demand signified a bid rather than an act of purchase. In a text quite recently from the press we find this thought stated in the words: "By the demand for any commodity the economist means in general the quantity of that commodity which buyers stand ready to take at some specific price"; it being added that "if we take care not to confuse demand with the amount which people want or need, we must be equally careful to distinguish it from the amount actually bought. Demand in the correct sense might be characterized as potential demand; the amount bought, as realized demand." [5]

The question whether supply should mean total stock of goods or only as much as was offered for sale, say at a minimum price, was likewise settled before very long, and that in favor of the second construction. But another point of scarcely less import was ignored in the price analysis, namely the possibility that each party in an exchange might be designated as either supplier or demander, seller or buyer. Because money was the regular medium of exchange, and sale for profit the aim of

[4] See for instance Fetter, "Economic Principles," 1915, vol. 1, and Jevons, W. S., in his "Theory of Political Economy," p. 119, where demand means purchase.

[5] Taylor, F. M., "Principles of Economics," 1921, pp. 253-54. See also Seager, H. R., "Principles of Economics," 1913, p. 73.

the entrepreneur, it was easy to forget that after all the relation between dealers was a reciprocal one, both playing exactly the same part if barter for personal consumption of things bought was the rule. Barring the definition of supply as a store of goods in the physical sense, supply had to mean supply on conditions, or in other words, a *demand* for a quid pro quo which constituted the price. The demander on the other hand *supplied* either a *standard* value in goods, or any good exchangeable for something else. In barter the true relation between the exchangers would appear as it could not in a pecuniary régime where the entrepreneur pursued aims in principle different from those of the buyer for consumption. By a purchase of goods others had to be sold; in selling goods, others were inevitably bought. This was the dual aspect of supply or demand, of buyer or seller that must not be lost sight of in an appraisal of the method by which orthodox economics arrived at its law of price, respectively of income.

Demand and Price.—The failure of psychology in any form, and notably of sensationalism, to provide us with a law of price, whether we think of an ordinary business transaction or of an exchange of goods for the personal use of the exchangers, may then be explained as follows:

In the first case, namely, we are confronted with the undoubted fact that buyers and sellers act from different motives and represent modes of valuation so different in kind that they are really incomparable. To attempt therefore a reduction of prices to sensations or to feelings or to any psychic condition, proposing a measurement of one by the other, is to invite criticism as well as to court bitter disappointments. The buyer who

resells what he bought has nothing in common with the consumer-buyer, but much with the seller who is in the business for gain. For both of these dealers are actuated by a desire for profits, and not at all by an interest in the use-value or utility of what they deal in. That the buyer for personal use of goods has preferences and margins of wants is evident. But this does not affect the other two parties. No pain-pleasure calculus is applicable to buyers for resale or to sellers. Financially they may be subject to motives that a pure theory of consumption ignores. The utility of sellers is not technical, like the consumer's, but entirely one of earning-power. However paradoxical it may seem, the technical utility of goods used in production by the manufacturer who offers his wares for sale has nothing to do with the worth he puts upon his business or his individual goods as a basis for net profits. Nor can, incidentally speaking, the purchasing-power of consumers, which has long been recognized as playing a decisive rôle, apply in the least to the buyer for resale or to the seller. In an entrepreneur world, indeed, buyers and sellers stand for such different principles of valuation that a quantitative relation between values as pleasure, or of any other sort, and the prices of goods is impossible. Marginism was bound to be in the wrong to the extent that its explanation of price rested on a psychological analysis, and this regardless of whether it used ancient or modern theories of valuation.

But suppose we imagine a world in which people trade purely for personal advantages of gratification, without aims at business profits, and even without the use of money. Under such conditions, what would be the facts

bearing on a law of price? How would consumption goods be priced, and what factors would have to be considered for the formulation of a law of price, if it exists?

At the start it deserves noting that two questions are involved which make different demands upon our time. For the first would be: Why are goods exchanged, and the second: At what rate are goods exchanged? These two questions are indeed quite distinct, and not to be confused in a search for principles of pricing.

The first assumes no more than a desire on the part of men to add to their pleasures by an exchange of goods. If there are two persons dealing, and each wants the other's goods more than his own, different kinds of articles will be "swapped." Boys trade different kinds of knives, or a knife for a popgun in this manner. No psychic states need be *measured* by us in order to explain this exchange. All we admit is the hedonistic principle and *some* difference in want by each for the article held by the other. Orders of preference may become evident as the number of things exchanged is increased; but the exact degrees of valuation *entering into the rate of exchange* may be of no moment.

The matter is different, however, if instead of one pair of dealers we have two or more dealers on one side bidding for the goods of one dealer on the other side, or of several dealers on the other side. For now a measurement of relative wants must take place that is decisive for the price at which goods are traded. It is not the comparison of wants between buyer and seller that counts, but the comparison of wants among buyers on the one hand, and among sellers on the other. The rate at which goods will be exchanged depends on the amount

of a given good which each of the buyers or sellers will offer for the desired good of the opposite side. This *relative* bidding-strength of all the members on each side settles the price; and so the *absolute* inequality of want of any one buyer as against any one seller has, from the standpoint of the economist, only a secondary significance. For what is studied is price, not the advantage of exchange as such. It is to know how price is determined that we search for facts not relevant to an explanation of commerce itself.

But this being so, what follows for the psychological argument of Marginism, indeed, for any correlation of price with psychic data?

If we assume all traders to possess equal stocks of goods we must take their preferences to be purely personal. Offerings of high prices, i. e. of large amounts of goods, for one article will be offset by correspondingly lower bids, in terms of goods, for other articles bought. By assumption total stocks represent equal purchasing powers, so that bids reflect wants and tastes only. These differences of preference may be innate, or acquired after birth. They may connect with diverse factors not open to inspection, or at any rate not at all measurable by known standards. As we have seen, furthermore, such valuations are highly complex things, and not to be resolved into physiological data or sense pleasures. What is back of each man's degree and order of wants, and whether these may be constructed into a law of valuation, is a question distinct from that of pricing. It has already been intimated that laws of valuation in the psychological sense may not exist, so far as our present knowledge goes. But it is certain that *wants*

alone figure in an analysis of rates of exchange among dealers with like quantities of goods.

Since such a condition however does not actually obtain, since inequality is the rule, the rate of exchange as one of amounts of things or services offered for a unit of other things or services varies with the degree or range of such disparities. What is known as purchasing-power becomes a factor of primary significance in the process. Intrinsic want-feelings or idea-valuations can no longer determine prices, for price is the amount of one thing given for a fixed amount of a second. Or to state the situation more precisely: *Like wants may now be measured by unlike standards*, the wants being personal-psychic, and the standards definite quantities of wealth held by each party in the transaction (bearing again in mind that we have assumed barter and exchange for personal use, not for gain by one or both sides). On the principle of diminishing utility it is then safe to predict that a man will offer the more of what he has for something desired, the larger his total assets; and vice versa. Not that prices must therefore be adjusted to the wealth of each individual buyer; for experience teaches us otherwise. We shall pay for goods a price uniform for a given region or group of people irrespective of our differences in wealth. But none the less, these differences of stock owned by each dealer help to determine what is bought at what rate, the members on each side competing with one another, and the price resulting from this measurement of wants in terms of purchasing-power *then* remaining the same for all, regardless of differences in wealth among the traders. Price, in other words, is a resultant of many bids, of different numbers of traders

for each bid, and of different amounts of goods covered by each bid. This is the smallest number of elements to which we can reduce price determiners on the demand side. What is back of these bids is a separate question. As shown awhile ago, it will depend especially upon one item, to wit the way in which items of wealth—on our present supposition, amounts of goods—are distributed among traders. Since purchasing-power has by long experience been proven to be a chief factor in bidding, and since psychic states are not themselves measurable, Marginists have from the beginning used bids or price as the proof of degrees of want. Or to quote from Jevons: "The price of a commodity is the only test we have of the utility of the commodity to the purchaser." [6]

But let us note a few further facts before closing with the demand aspects of price.

In the first place, namely, we must repeat that purchasing-power is only one determinant, albeit an important one. It does not follow that because total stocks are a function of the relative usefulness or value of any one portion of it, therefore they are the only function. Rather, we must be prepared to consider other items in this valuation, as already indicated; and *these others need not be at all psychic.* Climate or nationality or age or occupation or anything else may serve as a key to bidding just as well as differences in wealth. What is meant to be emphasized here is merely the necessity of knowing something about the distribution of wealth—in terms of money or not—in order to arrive at a law of price which has many determinants, one of which is the range over which purchasing-power is scattered for all parties in

[6] Edition of 1879, p. 158.

the exchange. What critics have called the status quo, relative to which the orthodox statement of pricing is true, is partly this distribution of wealth at a time.

But in the second place, do we really mean all wealth in defining this purchasing-power, or only a part of it? To carry out our hypothesis of exchange without money, for personal use of the goods exchanged, we had to start with the stock in hand as representing purchasing-power. But in the existing régime of money and exchange for profit by sellers, do *aggregate* stocks we own help to determine valuations and thus prices (on the demand side), or instead certain portions of it? Now, while this question cannot be categorically answered, it seems reasonable to believe that income rather than total assets is of importance, and that such income means to buyers for consumption an *annual* flow of value or purchasing-power rather than income for any other time-unit. Our total belongings are not likely to influence us, first because our feelings with regard to much that we are nominal owners of are not very lively; secondly, because of our inability to make any kind of estimate of such stocks; and third, because of the force of habit and our disposition to look more to future income than to values already acquired.

Finally, some difference between a non-pecuniary scheme of valuation and the pecuniary must be granted since money has unique functions, thus inducing us to value it as we value nothing else. "The valuation of a sum of money as a whole," it has been observed by one writer—and others have expressed themselves similarly —"where the separate instrumental judgments are suppressed, where its indefinite applicability to condition and

personal worths is assumed, and where it is referred immediately to the personality, gives to the sum of money, as a unity, an intrinsic value which may greatly exceed its actual value."[7] Money represents a special case of value and of valuation because it is a universal denominator, a magnitude divisible into smallest particles, exceedingly attractive as a counting device, and withal a store of treasure whose enduring qualities compare favorably with most of the commodities purchasable for it. Hence it must be acknowledged that our estimate of a fraction of a two thousand *dollar* annual income need not be exactly that of the same fraction of two thousand dollars paid to us annually in *goods*. But this of course does not affect the main point of the argument, viz., that differences in wealth or income figure prominently in the making of prices, while psychic facts as such do not. Demand therefore is something different from what either sensationalism or any other psychological theory of price laws would have us believe.

Supply and Price.—On the supply side the determinants of price ordinarily mentioned have been supply itself, and cost or expenses. Not that these three really were treated as distinct factors, but that they were opposed to demand; cost or expense acting through supply precisely as purchasing-power might have been, and by some was, described as operating through demand. What then shall we say of these determinants, or of others that may be detached from them, and yet have validity in a law of price?

If supply is to mean what so frequently was said of

[7] Urban, W. M., "Valuation," 1908, p. 340. For like statements see Simmel, G., "Die Philosophie des Geldes," 1907, pp. 272-94, and Elster, K., in Konrad's *Jahrb.*, 1921, p. 515.

it, to wit, an offer of goods subject to a certain minimum price,[8] then it cannot surely be called a "determinant" of price; for it would be merely a quantity of goods exchanged at a price—which is a very different thing. And we may add à propos of this thought that the so-called equilibrium [9] of supply and demand on those conditions was no more than a truism to the effect that what is bought is sold at some price. That of course would be incontestable, but could it deserve the title of a law? Indeed, we might further declare that such a statement was nothing but a corollary from the well-known premises of hedonism and mobility of capital and labor through freedom of contract. Supply at a price would be demand as well as supply, and could not be considered as a new factor among those determining prices in general.

Thus we are driven to the conclusion that supply, to fill a definite need, must mean physical stock of goods or offerings of services to a given amount. If supply means this, we may wish to find out whether it keeps a more or less fixed ratio to prices, for instance so that prices rise in inverse proportion to supply, or fall twice as fast as supply increases; and so on. Such a correlation is perfectly legitimate and has been attempted in a few cases both in England and elsewhere, but since supply cannot properly mean anything else than this volume of goods on hand (rather than market offerings at a price) it

[8] Vide Taylor, F. M., "Principles of Economics," pp. 268-9. Jevons, in his "Theory of Political Economy," 1879, pp. 70, 77, stresses supply per time-unit.

[9] Similarly, to say "the ideally just price is one which will secure a balance between production and consumption" is to beg a question, unless a just distribution of income be previously defined. See Carver, Th. N., in *P. and P. of Am. Econ. Assoc.*, March, 1919, p. 250.

follows that supply need not be that of any given moment. A static view of the pricing process becomes unnecessary, not to say illogical, as soon as we reject the psychological approach and take men, merchants, consumers, and wares-for-sale as they really function everywhere about us.

But with this understanding physical supply may also be profitably connected with other facts which act directly upon it, or may prove to move in some more or less constant ratio to prices. Thus prospects of gain do influence producers in fixing output and supply. Thus our valuations may lead to increased output even though rates of return diminish. The more we value something, the harder we work to get it; the larger the amount of capital-goods and of labor-power dedicated to its production. Again, monopoly or non-reproducibility may affect supply; or even more definitely cost or expenses, as has always been emphasized by economists.

Cost.—Cost in this case cannot however mean labor-pain or disutility,[10] since neither is measurable. We may point to them as elements in a qualitative analysis, but cannot use them to establish a price, *which* is a *definite quantity* of two or more goods exchanged. Cost, then, must be made objective. It must signify a physical volume of goods destroyed, as for instance seed-wheat by the farmer, or the coal burned in smelting iron-ore. Such a correlation is permissible, although we have decided long ago that no law of exchange-value is discoverable by that route. Neither does outgo of time or of muscular effort seem to account for the fluctuations of price,

[10] On the pros and cons of the disutility notion in Marginism see Schumpeter, J., "Wesen und Hauptinhalt der Theoretischen Nationaloekonomie," pp. 221-34.

and furthermore, labor-costs and other costs are so varied and complex that a measurement of them seems out of the question. Manual labor is not at any time comparable with mental labor. Joint-costs cannot be imputed in exact amounts to the several products resulting. We have fixed versus variable charges, reproduction as against production costs, and minima offset by maximum costs—always in the non-pecuniary sense. How may such data be brought into exact relation with prices? That it is for the future to find out.

Expenses.—Similarly must expenses be regarded as facts difficult of measurement, although an attempt should be made if a law of price is to be established. As *monetary outlays* expenses can certainly not be called determinants, since such facts are themselves values or prices, belonging either to the past, or figuring as estimates by the producer or merchant who looks ahead in order to conduct his business. As past or prospective valuations these expenses can mean nothing in a search for laws of price unless we *exclude net profits* and seek to correlate statistically the remaining sum with final retail prices, or with prices resulting from all immediately preceding expenses, minus net profits. That all expenses *in*clusive of net profits must equal the sales price is a safe guess, except where absolute losses are incurred. But such cases would be rare. On the other hand, if expenses not including net profits were to show a fairly fixed quantitative relation to prices, that would give us a law such as any statistical method may lead to. Only, we should have to remember that, as with costs, so here too the technical obstacles in the way would be often insurmountable. For once more there would be joint versus single

expenses, fixed charges, maxima and minima or perhaps averages and in addition the differences between actual expenses of production and these plus incidentals which, in the present economic system, are at times far from negligible. What criterion is to guide us in such a perplexing situation? Would it not be more a matter of definition than of scientific method?

To conclude therefore our survey of Utilitarian and Marginal pricing, the first stressing psychic facts and the second the rôle of costs or of expenses: We shall have to admit first of all that price is a resultant of far more facts or events than our traditional analysis has permitted us to understand. We have no right to picture all elements as working *through* supply and demand; but we must on the other hand be willing to consider a variety of facts physical and otherwise, if a law of price is to be discovered. Secondly, there is good reason for using psychic facts in a qualitative analysis which shows *why* prices exist, and why they differ; but this is not to vindicate those who proclaim preferences and intensities of want to be the final key to prices. Third, a correlation of one group of prices with another is in order, and may net us as satisfactory a law as possibly any other method. This it remains for us to investigate, although *non-price* facts must always be accorded a prominent place in either a quantitative or qualitative analysis of prices. Fourth, with regard to special problems, such as the market value of production-goods of lasting qualities, or of labor fighting for a standard of living—with respect to these both our enumeration of determinants and their measurement relative to price must be much more comprehensive than a science of catallactics could have

suspected. An abstract price analysis has the virtue of simplicity, but what if this is its only one?

The Marginal Concept.—Having disposed, then, of the fundamentals in Utilitarian and Marginal pricing we may now proceed to a brief consideration of the margin which was designed to give the subjectivistic view of economic processes a most precise appearance, and in fact accounts for the name Marginism or Marginal economics.

Utilitarianism had introduced two kinds of margins, namely one to determine (or measure?) rent, and the other to show what amount of expenses determined the prices of commodities. The Ricardian idea of rent, since it found the key to rent in productivity rather than in monopoly, had to take account of different soil fertilities either as such, or as returns in dollars and cents. No-rent land thus was differentiated from rent-bearing land which represented all yields more than equal to a subsistence fund for the tenant. How much rent might be paid, and normally was paid, to the landlord depended upon the difference between the return of the worst land in use and that of a superior piece of land. In this way land at the margin became most important for the calculation of "economic rent." Similarly the prices of goods corresponded, not to an arithmetical average of all expenses incurred by different producers, but to either maximum or minimum expenses; to the former in a short-time view, and to the latter in the long run. This was the decision handed down by the classicists, and accordingly least efficiency turned out to be a decisive margin, since orthodox economics always took a static view of the production and pricing process.

Now, Marginists had to go a step further than their predecessors because they had committed themselves to a subjectivistic interpretation of value or income. Since they traced exchange-rates back to psychic states, to utilities or wants or pleasures or disutilities, the noteworthy fact for them was the difference in degree of pleasure or want in different people. There was need of recognizing first, various intensities of satisfaction, secondly an order of choice which should make the unit of pleasure in using one class of goods nearly equal to that gained from the next preceding class, and third a variety of uses of which a given article might admit. Thus there arose three kinds of margins for the pricing of goods, although all three had of course a common origin and depended ultimately on one single law, viz., that of diminishing utility or pleasure or want. (The three terms were used interchangeably.) For the distributive analysis other margins were invoked, of which more anon. But just now let us note that in the resort to this least want or utility men hoped to have explained price. Least gratifications or wants, it was said, determined what would be paid for an article (or a service!). Goods were bought so that the pleasure derived from the last unit of one good was nearly equaled by that derived from the first dose of the next good in order of preference. Man's hankering for maximum pleasure was responsible for this arangement. In the balancing of such magnitudes of value or pleasure all exchange had its origin. As for different uses of any one commodity, the least valuable would inevitably figure in its rating when combined with other things, or when used by itself for a greater satis-

faction.[11] The least want was always the decisive element, regardless of differences of gratification experienced by different buyers. Hence also these differences could not mean different prices, for the hedonistic motive would protect the supra-marginal user or buyer. This applied to the demand side. On the supply side the retention of expenses as a determinant of price assigned to least efficiency the same rôle that Utilitarians had granted it. In a word, margins for both groups of economists served to explain prices, precluding the possibility of more than one price in an open market, and connecting price with a differential that was taken directly from the realm of facts.

But what shall we say today of such margins as a determinant of price, as an avenue of approach to a law of price valid for all times?

In the first place we need of course not dispute the existence of differences, or the merit of distinguishing between first and last sensations in an act of consumption. That we equalize our pleasures frequently, and usually to a degree, and that we gauge the extent of a loss not by the greatest pleasure the article gave us, but by the least sacrifice which will replace it—these points may readily be granted. We may object to having disutilities confused with utilities foregone, or to having theorists insist upon a law of one price when our ears and eyes tell us of several prices in our home markets on the same afternoon. But these are details.

In the second place however we have already seen that Marginal psychology as a whole was wrong, and more

[11] Due chiefly to Wieser, F., in his "Natural Value" (transl. of Malloch, Ch. A., 1893). See p. 98 *et passim*.

particularly that psychic entities remain incommensurable. This is true beyond doubt and interferes seriously with the plea for margins. Further, and in the third place, the law of diminishing utility likewise has its limits, as stated in the previous chapter. The Marginists themselves have pointed them out and accordingly minimized the worth of their standard. And this inapplicability of utility-margins becomes the more striking the more we consider our stock of goods as a variety of wants, or as a pecuniary income, for both of which value-judgments prevail that have nothing in common with sensations.

Fourth—and perhaps most to the point—the hypothesis of a margin helps us in no wise to formulate a law of price, for the question still remains: Why did marginal wants or values represent such and such a magnitude? To be sure, if psychics were measurable we might let the measured limen of gratification stand as a last cause of price. But inasmuch as our feelings and judgments cannot be so definitely ascertained, we must look elsewhere for an explanation. We must find out why want or market-bids, and not pleasure or utility, moved on a certain level or declined to a certain minimum decisive for price. Objective causes and correlates should be established if a law of price is to appear. As long as this is not done, the mere discovery of a least dose will boot us little. Though margins therefore did mean a refinement of analysis, they provided no ultimate scientific explanation. They were a device for dialectics, a fiction convenient for debaters and mathematicians engrossed in "functions," but hardly a solid basis for generalizations.

The margin, in fine, added nothing to subjectivism in general.

CHAPTER FOUR

DISTRIBUTION

Preliminary Definitions.—If economists had treated incomes consistently as prices, without bothering about forces other than the psychological in their attempts at explaining these incomes, a critical review of the price analysis would suffice for the distributive aspects also. It is however wellknown that few writers were content with a discussion of income laws entirely from the standpoint of demand and supply. What seemed to impress all of them was the necessity of bringing in non-psychic elements in order to find laws for shares comparable to those of commodity prices. Even Marginism, which labored most conscientiously to make of economics an exact science by relying upon the hedonistic calculus in all its arguments, could not avoid at certain moments to admit objective norms. Distribution therefore is not altogether a special case of pricing for goods. There are facts to be considered which a rejection of the sensationalistic theory of valuation does not touch, and relative to which one is prone to ask more than ever: Were laws of distribution really found, or was the qualitative analysis, in spite of special aids, again a half-way procedure? Certainly, if one is to believe the literature on the subject, some very creditable results were obtained.

Both Utilitarian and Marginal economists delimited the

scope of Distribution as a special division along two main lines. The first was the exclusion of data not within the competitive exchange mechanism; and the second, the assumption of specified legal conditions thanks to which all producers could be assigned to one of four classes. The first of the two principles was the most important because of the definition of "economic" that it involved; but the second was emphasized more regularly, for it led to practical questions that critics were not slow to take advantage of when the need arose. It might not strike people as anything remarkable that economics should be a science of catallactics, but it was bound to arouse interest that in reality there could be only four claimants to the wealth produced by a nation. To define economic income was to state merely the amount to be divided, and what it did not embrace. But the legal premises, which were familiar to all, designated the sharers in the product, emphasizing their rank and social prestige in some measure; and that might well become a popular issue.

Let us note at the outset, then, that economists always distinguished between a dividend, its sharers, and the share-amounts. The dividend from a common sense standpoint would probably be the total income of a nation in goods and services, regardless of whether all of it was offered for sale and thus exchanged at a price, or not. But for a science of catallactics that of course could not be the definition. By "dividend" for purposes of finding income-determinants was meant exclusively such wealth as entered the open market. Two sources of income therefore were ignored by orthodox analysis, viz., first non-competitive payments in kind resulting on the one hand from certain legal relations or on the other from produc-

tive effort such as the work of married women, children, and other people productive, say, after business hours; and secondly incomes not earned by personal effort, or at least not directly connected with it, but none the less accruing to people on occasions. Thus inheritances, gifts, finds, endowments and other instances of charity, life-insurance, bonuses on various grounds, and the results of gambling, all these ways and forms of acquisition remained necessarily outside of the distributive scheme of economists.[1] Whatever portion of the grand total of property-transfers was not due to production-for-exchange,[2]—and it is still considerable, possibly is on the increase—escaped examination, it being not adaptable to a hedonistic principle of a definite quid pro quo. An unworked field of such extension might, to be sure, be considered a regrettable defect in any theory of distribution, but the approach to income through price left no alternative.

Similarly the recognition of only four sharers might have been, and at times has been, criticized as a barren, if logical, abstraction of men too much bent upon weaving a system. It has been pointed out how artificial this four-fold classification of earners is, and how much more practical the periodic survey made, e. g., by the census-taker who finds out about personal incomes or family budgets, or about the financial standing of specified occupational groups. The official sharers of the science of economics

[1] For a list of income sources other than services within the exchange system see, e.g., Ely, R. T., "Property and Contract," vol. I, pp. 51-5. A well-known threefold classification of distributive processes is that of Clark, J. B., "Distribution of Wealth," ch. 2.

[2] Schumpeter, J., "Wesen und Hauptinhalt," p. 321; also an article in *Arch. f. Sozialw. und Pol.*, 1916-17, pp. 1-89. See also Wagner, A., "Theoretische Sozialoekonomik," 1907, Part I.

represent a proprietary régime in which owners were contrasted with the proletariat. Those who had land, or capital other than land, made up two classes. The captains of industry who might as such have neither capital nor land, but hired the use of them, constituted the third party, while the laborers they employed under contract represented a fourth contingent. Thus there were two sharers with property, one with acquired rights of management of land, capital, and labor, and one that did the work set before him by the enterpriser. Landlord, capitalist, enterpriser, and laborer made up the family of sharers in the social dividend. The law gave two of them a right to income from property, and the other two a right to income by personal effort. The manager of the other three sharers had no guaranteed income. He was in this respect the exception, and therefore proved from the beginning an embarrassing figure in the distributive process. However, it should also be remembered that a sharer was not necessarily a living individual, a real person; nor always the participant in only one of the four shares. For in the first place legal persons like corporations would secure a large portion of the grand total, and in the second place any one sharer might in his, or its own legal, person combine two, three, or all of the sharers. A farmer as entrepreneur (enterpriser) might hold bonds and get interest, work off and on for others, getting wages, and lease out part of his part, thus collecting rents. A sharer was therefore a *theoretical* entity. Many physical persons would make up one "sharer," and one person might represent four "sharers" as economists used the term.

Shares, as distinguished from sharers, were the amounts

going to labor, capital, land, and enterprise. These four sharers together would get the whole dividend as formally defined; and there could be nothing left over. But if we ask what the share meant, what the unit for income-analysis, the answer would not be: A portion of the total within the exchange-mechanism, but a fraction of any production-unit suiting definitions of value and production. How much of the national dividend each sharer procured could not be ascertained by the premises and mode of reasoning employed by economists, hence was of no immediate concern to them. It would of course follow that if all the shares for each particular transaction were added up, then the share of each of the four claimants in the whole national income would be measured. But this was only of incidental interest. What engaged the attention of students was the manner, the principle, by which any one product in the precise scientific sense was divided, and this led at once to a consideration of hedonistic premises. Since men sought maximum gain at minimum cost, since value was something (tangible or intangible) scarce, wanted, and legally transferable, and since production consequently meant the creation of values rather than that of things, the proof of production was an addition of value. The unit for distributive analysis therefore was either that *value created before the very first sale of a good or service took place, or that increment of value occurred between any two sales.* There were form and place and time utilities, and there was even a creation of value through effectuating a legal transfer of property rights, as in the case of a real estate broker. Thus acquisitive and creative norms, for one thing, might easily be confused, since according to orthodoxy the proof of

"production" was this addition of values, whether due to personal effort or not; while for another thing any one productive act might involve all four shares of profit, rent, interest, and wages. In the sale of a fountain-pen, e. g., the store-keeper would claim a profit; the clerk employed would get a wage; the owner of the building in which the store was located would get rent; and if somebody held a mortgage on the merchandise there would be a deduction for interest. This followed from the entrepreneur view, and hence a variety of conditions helped in each sale, in each productive act, to determine shares. Nay, on any but a psychological analysis the determinants for different productive acts would vary so much that a law of income could hardly be established. The objective approach to income was, in other words, *incompatible with the ideal of an exact distributive law, or set of laws.*

How then was the search for laws conducted?

The Ricardian Scheme.—As an excellent example of Utilitarian theory may be taken the Ricardian which, with some alterations that do not matter for present purposes, has survived to this day. Ricardo, we know, relied upon laws both of physical and of human nature, though the distinction was not offered in so many words. He borrowed from Malthus the idea of a subsistence fund, and he went to other contemporaries to formulate the law of rent that passes under his name. There being but three shares, the problem was neatly solved. For labor would get no more than was essential to a bare living and to the maintenance of a family. The landlord obtained the whole yield of land better than marginal or no-rent land. The pressure for the product (say wheat) being the cause of the cultivation of successively inferior soils,

the hedonistic proclivity of man and of the owner of land in particular precluded the chance of the produce being sold directly proportionate to expenses or to non-monetary costs. What was left was then profit, which included interest.

Variants of this scheme were to be sure submitted in the course of time, and Americans especially are acquainted with F. Walker's treatment of wages as a residual share. It was argued that labor got its own product minus the shares of land, capital, and enterprise. Rent was again the supra-marginal product, interest a reward for abstinence, and profit the difference between maximum and less than maximum expenses. The lower an enterpriser's costs (expenses) of production relative to the expenses of the less efficient rival, the greater his profit. It was a rent like the landlord's, albeit more of a contingent income because capital was not a monopoly like land *taken as a whole*. Labor thus claimed all it had produced after deduction of the other quota.

Now, an important difference will be seen to exist between the original and the later objective schemes of distribution. For according to Ricardo *physical* output was a true differential, states of mind being ignored. The masses received enough to live and to perpetuate their kind. The landlord had to pay the maker of capital-goods a price equal to maximum costs of production. This he reckoned as part of his costs of producing wheat when computing his rent. Even land next to no-rent land used some capital-goods. This land therefore represented the marginal strip, above which existed lands yielding more wheat, that is to say a surplus converted into rent. The owner of the soil, then, did no worse than the enterpriser

in the city. They both shared in a differential advantage of capital, immobile or mobile. It was the laborer whose share was at an absolute physical minimum, except when his employers in farm and factory, for reasons not here important, allowed him a mite above subsistence. But it was always physical goods that could be pointed to as the shares. A pecuniary or psychic measurement was not logically necessary, and besides Ricardo held that in the long run profits would approach wages, so that the sacks of wheat above those of marginal harvest formed the only residuum. Eventually "almost the whole produce of the country, after paying the laborers, will be the property of the owners of the land and the receivers of tithes and taxes." Thus was a scientific determination of shares made possible by the Ricardian analysis.

As soon as abstinence, however, was introduced as a cause for a fourth share, to wit interest, and as soon as costs were defined strictly as monetary outlays, the Utilitarian scheme lost its logical coherence. For a psychic entity was now measured by a price, namely an interest-rate, and in the second place expenses involved a begging of the question. The share of capital could not be said to be "determined" by abstinence since it was used as proof of a pain of non-consumption, the pain rising as the interest-rate did, and vice versa. Nor could expenses render the service that things did, since they were themselves values which had to be further explained through something *else* than value, if a real correlation was to be established between them and the price of services. Shares consequently ceased to follow laws in the scientific sense of the term. If a law of shares was still to be found inquiry had to extend to physical facts, not to values. Some

such facts were indeed adduced. Risk, for instance, came to figure in the determination of wages, or irksomeness of occupation, or again a standard of living; that is a state of mind or custom whose own explanation was not attempted because it lay outside of the price system. Not that there could be any objection to the discussion of these non-hedonistic elements. On the contrary, it has more than once been intimated that a formulation of price laws necessitates such studies. But we must also note that in so enlisting the aid of non-psychic and non-pecuniary factors, Utilitarianism confessed the fallacy of a purely psychic causation. Marginists should have been warned by this experience of their predecessors; but of course they were not. They went ahead serenely, hoping to achieve by a strictly psychic analysis what Utilitarianism had at first tried to do by a correlation of values with things such as labor-amounts, and later on had sought to accomplish by taking expenses objectively.

On Wages and Profits.—Deferring the consideration of margins for a while, and taking up first the shares of personal effort, i. e., wages and profits, what was the procedure of the subjectivists?

The analysis which sufficed for consumption goods, particularly individual utilities, might of course have been applied also to labor-services, and in so far as all incomes constituted prices nothing else should really have been attempted. But a number of elements peculiar to labor-prices were soon recognized and taken account of. To begin with, for instance, a standard of living was eventually admitted to interfere with a strict competitive interpretation of wages. Labor was not a commodity *merely*, or perhaps not at all—as the unionists would have

it. A minimum of allowances, irrespective of what individual bidders and a merciless enterpreneur régime would lead to, came to be accepted as a modifying factor. In the second place, economists at an early date had reckoned with objective data, even when Utilitarian standards did not call for them. While some harped on time or outgo of energy as possible determinants, others referred to different degrees of risk and disagreeableness, or to expenses of training skilled labor, or to the number of laborers available at a given moment for a particular class of production. All these and other correlates figured in treatises and to this extent betokened a departure from either an iron-law of wages or a purely psychological standpoint.

But an important circumstance, as soon as subjective norms were invoked, was the large amount of labor employed, not only to render personal services for the satisfaction of wants, but also as a means to another end, whether this latter took material form or remained itself a service. For Marginists, that is to say, the services of a butler or physician might be said to follow the law pertaining to consumption goods; but what a brakeman, e. g., or a mechanic produced who helped turn out furniture or tools for building operations could not so be related to want and purchasing power. Hence the resort, almost from the start, to a productivity theory; and hence the desire to compare or connect causally definite amounts of output and wages.

What however was productivity to mean in that case? Would it be physical things or useful services as such, or valuable items, or in short values? The issue was plainly a vital one and could not be dodged. Even though physi-

cal results[8] were meant, how could these be measured? Since most services were delivered in joint efforts yielding a single product, did not a problem in imputation arise that would defy the ingenuity of economists? This surely was understood by many and forthwith felt to be an insuperable obstacle. But on the other hand, if productivity was construed to be value-output, as it logically had to be, was not then the question raised what determined this quantity of value? When and why was a service, say the brakeman's or the mechanic's, worth so much, and in case of differentials among laborers with one employer or with many, or among such as were self-employing, which productivity was decisive?

As will be shown in a moment, when margins come up for consideration, this last question of differentials was carefully studied, and besides, there were the familiar norms of maximum and minimum expenses. But just now it is the dependence of productivity upon a broader price law that counts, and on this point Marginists had to acknowledge either ignorance, or a begging of the question. Wage-laws consequently could not be affirmed to have been found, even though they might exist.

And similarly with the question of profits, which so customarily were regarded as a residual.

An objection to this argument was, of course, the fact that net profits could not theoretically exist in static economics; for hedonism and the mobility of labor and capital under competitive conditions tended to level all shares, leaving instead of profits simply wages-of-management. This was the usual reply of those who inquired

[8] On use of mental measurements for finding efficiency and fixing wages see Woodbery, R. M., in *Quarterly J. of Ec.*, 1916-17, pp. 690-704.

with frankly critical intent into the how and what of profits. But in the first place, such wages-of-management would still offer the same difficulties of analysis as labor-prices, and in the second place there remained, in the real world, none the less a surplus above such wages-of-management. Thus the objection to Marginism was first that it could not explain a very large part of the income divided within catallactics, and secondly that final net profits could never be coupled with a sensationalistic or with any other psychological doctrine of valuation. Correlations of a statistical sort might be made, but that was unorthodox; and if against this it was urged that profits were aleatory, then again no law of profits apparently obtained. In either case the phenomenon of profits proved a profound, inscrutable mystery. And so possibly it is.

Rent and Interest.—But what of rent and interest as prices for services exchanged in an open market? Let us see.

Rent in many cases was a price for the use of land for its own sake, without any intermediate link of concrete goods won from the soil. As site for an athletic field or a private residence land might therefore yield revenues conformable to the general laws of price, supposing they had been discovered. But once more, not only was in that case the old psychological analysis inadequate for finding a law, but also most services of land satisfied wants only in a roundabout way, most often by yielding produce as food or raw-materials for production. Thus all the shortcomings of the productivity-theory applied to rent as well as to other shares. Physical output would never do, since supply in the physical sense had not been proven to be a

fixed function of prices. Value productivity referred back to values of the derived products, and thus to the fundamental problem in pricing which psychology could not solve. And in addition joint-values offered the unanswerable question of an allocation of shares in the final product. How could rent be considered explained merely because it was *defined* as a supra-marginal value? What did this boot if no law for the amount of surplus was found, if no events could be shown to accompany more or less regularly a stated sum of rental values? This side of the situation was in itself puzzling enough, but joint productivity was a further challenge to the economist. Nearly all land-services represented joined values. Costs as improvements of the soil might be absent, but labor was nearly always an ingredient in the product. How allot the several parts of the product so as to isolate rent?

Now, if such were harassing questions anent rent, the task of Marginists in expounding the principles of interest was even more exacting. In the course of time a great deal of labor was spent upon the problem, but really in vain because of a subjectivistic analysis. Besides, there was much confusion as to the term "capital," so that interest-rates seemed to obey, not one law, but several.

Interest was the price paid for the use of capital; this all agreed to. But what was capital? The word had several uses which did not all have the same bearing on loan-prices. From one standpoint capital was a production-good used to produce consumables, and this technological relation would exist whether private property were abolished or not, whether interest had a price or not. The old question that Karl Marx had raised might have been settled on this principle. We might contend that a

nation is interested only in efficiency, and that capital therefore need mean nothing else than a means of indirection in productive fields, a link in a roundabout process of production by which cost was greatly lowered and the wellbeing of the majority furthered.

By an individualistic interpretation, however, the thing-aspect would be subordinated to a value-aspect. Capital would become a basis of profits or a right to income, or a measure of expected differential incomes, and for this reason bear testimony to the acquisitive trait in human nature rather than to the creative.[4] If for the temporary use of my wealth I could charge the borrower in fractions of the physical amount of wealth, or in percentages of its money-value, relating my charge furthermore to time, this rate of interest might become a standard for measuring other kinds of income. Any kind of wealth might be capital, regardless of its use. The sum of values loaned out might be money instead of tangible productives or consumption-goods. And so, to be sure, it usually was. The entrepreneur system thought of money when it mentioned capital, not of technical production goods or of goods for personal gratification. Capital was a fund of values expressed in units of the standard currency of the country. Whether the borrower bought consumption goods with his loan, or yarn or printing presses, did not matter to the lender. He simply advanced capital, and received it back at the end of a year with an added amount representing the price for the loan. He received the "principal" plus five per cent of it for the use he had allowed somebody else to make of the "capital." Capital thus was a value-fund

[4] Several definitions of capital from standpoint of businessmen are given by Woolman, L., in *Am. Ec. Rev.*, 1921, p. 39.

measured in terms of money; nothing else. A lender's capital might be turned into consumption goods by the borrower; or this latter might purchase technical productives (production-goods) with it. The word "capital" would still be used for both groups of value, but that was unfortunate, and a natural source of misunderstandings.

There was however a third side to the question. For if money as a medium of exchange became capital when loaned out, bearing an interest the while, might not production-goods, since they were a lasting source of profits, acquire a value relative to the interest-bearing power of money, i. e., proportionate to the excess of net profits above the interest-rate? Could not a technical agent be credited with differential profits and thus assume a value above its original purchase-price, irrespective of whether it had cost anything or not? Couldn't intangible assets emerge that would reflect the prevailing interest-rate for, say, a hundred dollars?

The answer of course is that all this was possible and was done continually. The business world took the interest-bearing power of money for granted, and rated its technological means of production accordingly; that is, if they were not destroyed in one single act of production! Yarn would be capital both in the sense of being a technological means to an end, and as a value-fund which was borrowed at a price, or might be loaned out at a price. But being used up in the weaving it could not be rated otherwise. The power-loom, on the other hand, might be given credit for profits made by the enterpriser with the aid of other capital and of labor and land, and if the net profits rose much above the current interest-rate, the value of the loom was figured higher in proportion. What is

known as "capitalization" took place; and here we have a third meaning of the word capital. Differential incomes of business were measured by a standard that was the prevailing interest-rate for all loans, excepting money-rates.

Now, the economist in studying the *price for the use of value-funds*, or ordinarily in modern society for the use of money in one form or another, was not concerned with any but the second meaning of "capital." He did not deal with capital as technological goods, nor with such assets as capitalized profits. It was the supply of loan-funds that interested him if he was to account for that share known as interest. It was a question of finding out how this supply came into existence, and what law of price might be discovered regarding it.

This being so, one fact stands out prominently at the start, and that is that costs in the ordinary sense could not explain capital. None of the shares could be related closely to costs, as we have already seen; for labor and enterprise were not chattels, but the rulers of chattel,—to say nothing of the unique rôle of the enterpriser as captain of the productive process; while land had no costs in *so far* as it produced without aid of either labor or capital. So what was to be the cost of producing capital, or the principle that regulated its supply?

As remarked before, resort was early had to a psychological factor, namely to the pain of refraining from consumption. It was held that men suffered by not using immediately what there was in their hands, and that this sacrifice called for special inducements such as an interest on a loan. The Marginists did not add much to this notion except that they went somewhat further into the

psychological aspects of abstention, showing how our attention is riveted upon the present. Senior's theory thus became a theory of "impatience" or an agio-theory, as an Austrian dubbed it. Capital was at first identified with stocks of tangible wealth, including goods for consumption when used for pecuniary gain. Because of this conception of capital, and because such wealth in earlier days was no doubt literally "saved" as a reserve contrary to people's inclination to enjoy their goods-income at once, the doctrine gained currency that the loan-fund varied with the degree of thrift of consumers or of the enterpriser—notably at first the landlord—who might hire either field-workers or butlers at his option.

It became evident however, as improved methods accelerated production and freed men from the danger of a deficit, that saving alone could not explain the whole of capital-goods, nor much less the loan-fund with which Distribution had to reckon. Impatience-theories could not hold themselves, first because wealth and loan-funds grew out of all proportion to the pain of foregoing enjoyments, which originally was (pain, genuine) and secondly because this pain anyhow was no more measurable than any other psychic state. For one thing, then, the available stock of wealth or of loan-funds would have to be the index of impatience, thus reversing the causal order, and for another thing the discrepancy between wealth as a means of acquiring profits or interest, and the loan-fund itself, gainsaid the assertions of the orthodox Marginists.

For this reason, doubtless, other determinants of the supply of capital were considered. It was pointed out that government regulations affected supply because of a fix-

able ratio of the loan-fund to cash reserves, especially for money-rates. It was easy to show that banks manufactured credits, that is rights to the use of values and wealth, regardless of stocks of production goods and even of cash-deposits by their clientele. Costs of operating a bank would, to be sure, influence the price of its services, including that for loans; but business conditions, general principles of investment, trade abroad no less than at home, currency changes and more particularly gold movements, which varied with facts not definitely measurable, these and other elements would count much more. There is no need here of going further; for an explanation of interest-rates lies beyond the scope of our inquiry. What *is* important is the failure of the hedonistic concept of impatience as a key to the supply of capital. Indeed, to mix risks and banking-costs (expenses) with time-preference was unfortunate in any case, because of the incomparable kinds of psychics involved. And then, of course, one might add incidentally that the chief suppliers of capital pretended to no pain of abstinence. Business-corporations spoke of surplus and investment, but not of a longing for consumption; and banks as main fountains of loan-capital would have stressed nothing more than their natural desire to "make" as much as possible. But this aim to earn was not comparable to the time-preference of the average saver. Nor was there any doubt that the banks set the pace, actuated by their own motives, after which industrial or trading corporations offered funds for lending according as they thought the prospects for profits through enlargement of their plants better than existing interest-rates, or not. And as for the multitudes who provided the smaller portion of the loan-fund mainly

through bank-deposits, they also followed the quotations of the professional lender. Roughly, this came to be the rule toward the end of the last century, and due to this alone the psychological approach proved misleading. As an explanation of supply impatience was but a makeshift, a mere reminder of the indisputable fact that present goods are by many preferred to future values. But it could not be a decisive factor.

What could be said, then of the demand aspects?

In the case of a man who borrowed directly for consumption the Marginist would of course plead again for his agio-theory. He would show why the borrower needed the value-fund that bought for him consumption goods, and would derive from it the price. Yet it cannot be repeated too often that, since impatience was itself immeasurable, the price would once more act as a barometer for measuring the want-pressure, just as the price of any other good was understood by Marginists to reflect psychics. And so there was nothing but reasoning in a circle. The only possible use an impatience-theory could have was to show why interest was paid at all. But was this any less obvious, any less a platitude, than to say that interest was due to the scarcity of the loan-fund? Was it not a fact to start with, rather than to wind up with, that prices presupposed scarcity relative to want, the two being inseparable? Was not time-preference, in the *case of a borrower who wished to buy consumption goods for his personal use* when he had no money of his own, as natural as the craving of goods by a man *with* purchasing power? Surely, the bare mention of impatience could not provide the materials for a law. It would have been necessary to state the more or less constant factors ac-

companying impatience, the laws of a *degree* of impatience.

But, since borrowing for consumption was rare anyhow, Marginism did not dwell too long on it except where it sought to explain supply of capital. On the demand side refuge was taken, as in the case of land and labor, to productivity. It was felt that a purely subjectivistic analysis did not suffice. And so men in both camps, whether they believed in time-preference or not, followed the early hint of Austrian thinkers that the superior productiveness of capitalistic enterprise left a fund out of which the lender could be paid, and which the producer was perfectly willing to share with him. Since capital was a means to an end, and since the borrower expected to recoup himself by his sales-price, the proposition was up to the public. Or rather, no, it was not. For it was argued that the roundabout process yielded a surplus of things, of goods specifically traceable to the employment of technological agents bought or hired with the proceeds of the loan, and that therefore a dividend remained from which interest was taken. This surplus would, then, account for both the existence of interest, and also for its rate, productivity-changes being followed by corresponding interest-rate movements.

Now, this idea must be dealt with essentially in the manner of earlier critics. In the first place, namely, technological superiority was often understood as a physical fact, and that of course was a mistake, since no fixed quantitative relation between supply and price ever existed, or at any rate has not so far been established. A rough correlation no doubt exists. It may well be assumed that, for a short time and for a specified group of goods,

a sudden lowering of cost, a marked increase of stocks, would yield larger total values and incidentally ensure the producer a better profit because of his new differential advantage and the relative immobility of labor and capital. Besides, while wealth is distributed unequally, any new commodity may have an enhanced temporary "adventitious" value, that is one not measurable by sheer utility. Higher complementary values have been shown to emerge in this manner and to permit higher prices and profits. Whenever the demand for goods is elastic, and capital is employed in such novel ways as to ensure the enterpriser an appreciably lower cost, interest-rates may be raised since the borrower has compensations in the price. When technical superiority affords a differential, and not the absolute advantage for all classes of producers in availing themselves of the indirect method, an increment of profits appears, the anticipation of which will not only make interest possible, but also tend toward higher bids for the use of capital as a loan-fund. Still, rates of interest have not yet been correlated with differentials of technological cost, or with supplies!

In the second place, while output and prices for goods and hence for loans *might* be compared, it would often be difficult to find that portion of the product which was due entirely to the use of capital; that is, either to all capital used, or to such fractions as were considered by Marginists. Since most goods are joint-products, representing more than one sharer, the old objection would again have weight. Imputations would be made without being verifiable by precise measurements; for either as specific physical or as value productivity the share of capital would be indeterminate. The interdependence of agents

of production has usually prevented experiments of addition and subtraction for the measurement of a single agent's product. In the words of J. S. Mill: Such a procedure was doomed to failure and hence forced upon economists a deductive method, because owing to a "composition of causes" causes and effects could not be so separated as to be assigned to specific conditioning facts. The events of the social world in this respect differ absolutely from those observed by the chemist, so that no test exists for showing what a particular lump of capital has produced when linked with other factors of production.

But in the third place, was not the productivity theory subject to the same limitations that weakened the case for subjectivistic price analysis in general? Was it not evident that value-productivity regardless of "impatience" varied with the price-determinants of goods, where goods intervened, and that hence nothing was gained by the concept of productivity unless the laws of price had first been stated—which we saw is impossible by psychological analysis—and unless some degree of regularity for the price of the service in question, as representing a *definite* ratio, could be proven? What was the advantage of interposing a y between an x and a z, when all three were unknown quantities? Was not the lack of objective data as disastrous to a hope for distributive laws as ever? And furthermore, was not the creditor lending capital (rights) whose value was predetermined rather than computed later according to its technical effectiveness? So far as the causal aspect of the matter is concerned, this might well be said, and was said.

Margins. The question was complicated by the fact that here, as in the statement of the law of wages and rent,

a margin was introduced to give an appearance of exactness to conclusions, and also at times one of ethical import. The same psychology that prompted the Marginists to speak of marginal utility and value also led them to apply least or last quantities to the problem of distribution. As indicated, productivity figured in the price of services, and not of commodities, because these former were rendered so frequently in an impersonal way. Hence its place under the heading of Distribution! The margin however was not peculiar to Distribution, as everybody knows. It functions elsewhere in great solemnity. But since incomes were prices, nothing else could be expected than an extension of marginal reckonings over the whole field of value.

But how many margins were there? It will repay us to tabulate the margins used in the three principal divisions of economics (see Tables One and Two). It will be seen that there was quite a number, and that consistency was a hard thing to maintain in the midst of so many standards. The price-margins for consumption goods were the primary ones, so to say, the others being derived from them logically. Of course, if productivity was treated as a physical fact—and this happened often enough—price margins had nothing to do with it. But strictly taken productivities should have been values, that is events sprung from a psychic fact, namely from utility or gratification or want—call it what you will. The two views of productivity were not always kept apart, partly because of the Ricardian ancestry of agricultural margins, and partly because with respect to labor and capital the difference between a thing and a value was easily overlooked in an argument. But if margins were used on

the orthodox principle, only differential costs could furnish a physical standard. That is, in fixing prices at a long-time view, by minimum non-pecuniary costs, no violence was done to logic. It would only be necessary to find out whether costs directly or through supply *do* determine values.

TABLE ONE

CLASSIFICATION OF MARGINS USED IN MARGINAL ECONOMICS

I. Price

1. Margin as last consumed and least gratifying part of a good used at a given time.
2. Margin as least wanted good out of a stock of different kinds of goods.
2a. Margin as least gratifying kind of use of a good having several uses.
3. Margin as marginal value.
4. Margin as maximum (long-run minimum) expense of production (costs).

II. Distribution

1. Margin as maximum impatience for the consumption of a good.
2. Margin as least effectively used dose of capital (stock or fund).
2a. Margin as least valuable use of capital.
3. Margin as least effectively used dose of labor.
4. Margin as least effectively used dose of land.
4a. Margin as least valuable kind of use of land.
4b. Margin as worst land (physical basis).
5. Margin as least efficient enterpriser (dose of enterprise?).

NOTE: Margins I, 1-2a, and II, 1 are the only ones having a physiological-hedonistic basis.

Comparing the margins further, it is seen also that some shares were related to several margins, while others had each one margin. All shares except enterprise had a proportionality-margin; that is, when a margin was used, it was a last dose of any one share relative to the *amounts* of other shares. It was a question of ratios of either physical or value agents. But for land and labor the margin bore on intrinsic differences of productivity as well as on such productivities as would depend upon ratios

TABLE TWO

IMPORTANT MARGINS FOR THE DISTRIBUTIVE THEORY OF MARGINAL ECONOMICS

Land	*Labor*	*Capital*	*Enterprise*	*Remarks*
Worst Land	Least Efficient Laborer (per Class?)		Worst Enterpriser (per Class?)	
Least Valuable Use of a Given Piece of Land		Least Valuable Use of { All Goods / Capital as a Fund }		
Least Effectively Used Dose of Land	Least Effectively Used Dose of Labor	Least Effectively Used Dose of Capital	Least Effectively Used Dose of Enterprise (?)	
		Maximum Impatience for the Consumption of Goods		
Above Margin	*At* Margin	*At* Margin	*Above* Margin	Point at which Price is Fixed
Land a Monopoly; Consumers' Demand	Labor Mobile and Dependent; Enterprise for Maximum Gain	Capital is Reproducible and Convertible at Will	Enterpriser Seeks Max'm Gain; Consumers' Demand	Reason for Point at which Price is Fixed

of the agents operating jointly. Again: The units to which the margins referred were not all the same. It might be a single productive act, or the output of a whole plant, an acre of ground or the farm-land of the entire country. Whether the choice of one or the other unit made a difference for the argument, or whether it was of no moment, could not be easily inferred from the actual use made of the marginal concept. And finally, there remained the interesting fact revealed in the Table, and long understood by economists, that rent and profits moved *above* the margin, while wages and interest were fixed *at* the margin. So this margin really had two functions, not one. It would be worth while to show why the shares had to, or could not, coincide with marginal productivity. Was it not the poverty of labor and its theoretical mobility that made the least productive man set the pace for all others? Was it not greed that won in enterprise, and failed in workingmen? Was it not the convertibility of capital as a value-fund into anything, and its reproducibility, that gave it a place at the margin when landlords could go on exploiting their monopoly or at any rate the unique properties that Ricardo spoke of? The intrinsically worst or worst used, land was the beginning of rent for all superior outputs, but the least effectively used capital provided no surplus. The rate of interest was fixed by the latter, if we believe the productivity economists. And the enterpriser? He was his own boss. He was a legal factor like capital or land, and therefore got the benefit of differential advantages which a laborer lost by being replaceable, within his class, by another fellow. Proportionality when well observed helped labor only in one respect, viz., by increasing the na-

tional output. But it gave the enterpriser two profits, viz., first in letting him share with the others the increased national output, and secondly in allowing him, within his own field or plant, to keep the surplus above maximum costs.

But apart from these inconsistencies in the use of terms and arguments, there remains to be noted the difference it made for the Marginal analysis whether we think of natural productivity (in things or values), or of proportionality. That lands differed in natural fertility, so that the yield of the better might be reckoned by that of the worst, was one thing; but that two like acres could be put to such different uses, through choice of crops or proportions of technical agents, that one became marginal and the other supra-marginal, that was a second thing. The worst land physically might not be marginal if treated better technically than land naturally more fertile, so that our standards for measuring shares overlapped.

And similarly with respect to the *unit* of the productive act that furnished the margin. Was it a single productive act such as the building of a house or the manufacture of a pair of shoes? Or was the output of the whole plant the unit for detecting the margin, for measuring the values above it? Ideas on the size of a market, and time-units in addition, should have been properly related to this assemblage of margins—if feasible. But neither for intrinsic nor for proportionality productivity was this step taken. Indeed, could a margin of any kind be found on the dosing principle? Did business calculate in this manner? Did it experiment so painstakingly as to have a fairly accurate record of relative yields and imputable incomes? It was granted by theorists that their argument

involved a picture for ratiocination rather than for verification. And one is constrained to accept the apology.

But finally, the margin was useless not only because it presupposed a wrong psychology of valuation, that is a thoroughly mistaken idea of law and causation, but because, once more, marginal productivities were as little measurable as absolute productivities of any one agent used jointly with others. Whenever producers worked together, whenever by-products were in question, whenever values belonged to the group of complementaries, the hope of finding a margin of productivity was a forlorn one. Hence the marginal standard could not do what hedonistic theories as such could not do, and the problem of income remained unsolved.

CHAPTER FIVE

PRODUCTION

The Place of Production and Consumption in Economics.—It has often been acknowledged that the science of economics dealt virtually with nothing else than price and distribution. It was in these two divisions that laws were sought and formulated. On the exposition of their salient points the foremost economists spent most of their time. Once price and income had been treated the pretense to exact science in one sense vanished, for few definite laws of production or consumption were ever elaborated. Consumption, to be sure, gained importance with the rise of collectivism and again under Marginism. The Historical School saw in studies of consumption a special method for suggesting political remedial measures, or for bringing out forcefully the bearing of thrift on progress, if not on taxation. The socialists favored the consideration of consumption as a logical division in economics, the term being in all cases interpreted as a physical or psychic, but not as a value, fact. Consumption from Naturalism upward meant use of wealth and precepts for conserving natural or national resources. Only with the entry of Marginism did consumption assume a psychological meaning which at the same time became an integral part of the price analysis. It was Jevons, e. g., who

wrote: "Economics must be founded upon a full and accurate investigation of the conditions of utility; and to understand this element we must necessarily examine the wants and desires of man. We shall first of all need a theory of the consumption of wealth." [1] In other words, Marginists identified a theory of valuation with certain physical and intellectual aspects of use and reaction or responses, thereby adding to the old conception of consumption a new one. Consumption was held to mean valuation mainly with reference to such acts of gratification as the inward and outward use of scarce goods brought with it.

However, consumption did not really become a well recognized portion of a science of economics, in spite of some notable attempts in that direction. The principles of valuation were treated under value or price, and that ended the matter. If anything else was written it fell under the rubric: Thrift or Taxation, or Wage-Earners' Budgets; etc. Nor did Production expand in the degree that the beginnings of economics might have suggested. The victory of Utilitarianism robbed the physical, collectivistic view of prosperity of its prestige. Thereafter individualistic, entrepreneur concepts held men's attention. It was an examination of price and shares of the national dividend that fascinated most minds; and so the division that had first preoccupied the Naturalists dropped out of sight. There was a good reason for it, too, and one which is not hard to guess. Namely, the trend of economists was toward a formulation of exact laws, of laws built increasingly on human nature. And how could Production compare with Value in this respect? The laws of produc-

[1] "Theory of Political Economy," 1879, p. 42.

tion were either physical or they pertained to values. If the former, could they be found? If the latter, was the avenue of aproach not a diagnosis of pricing processes? The situation was clear enough.

Production consequently could not mean much to a science of economics as built up with the aid of eighteenth century logicians and psychologists. As a division it remained noteworthy only because it gave rise to a number of basic ideas that were used over and over again in the analysis of value or distribution. Since these fundamental definitions ordinarily found an initial statement under the caption Production, and since by this step the field of economics was more or less explicitly delimited, and a road paved for the announcement of economic laws, Production must even today interest the historian or critic. But otherwise it involves no new problem. That economists devoted many chapters to the description of an existing régime, and thus had much to say à propos of Production, is true. The space assigned to this division is ample in the larger works. But one cannot overemphasize, at the outset, the fact that these long chapters describe merely a prevailing business organization, or treat of practical questions whose solution could never be seriously offered as a part of economic laws. What is genuine science in most books on Production fills but a few pages; the rest consist of a review of facts as facts.[2]

On Laws of Production.—One law of production was evolved early in the history of economics and may be reviewed before passing on to items of definition; and that was the law of productivity. There were in fact two

[2] For a recent statement on gaps see Woolman, L., in *Am. Ec. Rev.*, March, 1921.

distinct laws of productivity which only through an oversight, or because of a certain historical interrelation, paraded sometimes as one. To wit, there was the thought that, for example, an acre of land would in the course of time yield less, this phenomenon being originally called the law of diminishing returns; and there was the very different notion that things used jointly must be used in definite proportions at a given time and place, if the best results were to be secured. The two versions not only differed appreciably, but should without exception have been kept distinct.[8]

The law of diminishing returns referred of course to physical output. It spoke of things, and not of values. If a law of physical returns was to extend also to price it could be only on the assumption that a more or less fixed quantitative relation existed between physical and market supply on the one side, and price on the other side. This supposition was common enough, and had a foundation in facts. But what Malthus and Ricardo and many of their disciples in England, on the continent in Europe or in America thought of was the relative growth of population and of food supplies. Studying this ratio, the well established fact of soils becoming exhausted (if great care was not taken) assumed a sinister aspect. The law of diminishing returns created a sensation! At the same time it was of course also known that any plot of ground will yield a limited amount, so that the needs of a whole nation could not be supplied from one acre, no matter how diligently one cultivated it. But this law of limited physical returns, if one may call it a law, had no purpose

[8] Various concepts of productivity are discussed by Liefmann, R., in Conrad's *Jahrb.*, 1912, pp. 273-327.

in economics beyond helping to explain the scarcity of want-satisfying things.

Considering that Utilitarianism dealt largely with things, as opposed to value concepts, there was logic in its treatment of a law of physically dwindling returns from a long-time view. But when this law was understood to refer to less than proportionate increases of wheat relative to certain increases of labor or capital in the working of the soil, the thought arose naturally enough that perhaps land was not different from other agents in this respect. The question could be, and was, raised whether capital would yield always in proportion to added amounts of land or labor; and the reply was in the negative. So, after a while, diminishing returns as a theory of supply relative to population lost prestige, all four factors of production being placed on a par. Yet it should have been remembered that since all goods came ultimately from land in the wider sense, the declining rate of yield of land had a deeper significance than any law of the proportionality of returns. For plainly, rising returns in machinofacture or elsewhere could not avail, if the farm or mine continued to yield less on account of exhaustion, or boasted only of a stationary output. But perhaps this was felt to be a dynamic view of production that had no place in a generally static analysis of values.

As regards the exact law of proportions, however, this had weaknesses that it could not take long to point out, and for which no remedy was in sight. For instance, if we accept it at its face value, it was a statement of proportions of things necessary to produce the best results. The intimation made is that definite amounts of things have to

be used if maximum efficiency is to be attained. But when we look a little closer, asking ourselves what these "things" are that must be used in fixed proportions, we shall have difficulty in defining them. If productivity was output of values, we may or may not assume that the producing agents also represented values. It need not follow, though we might decide to that effect. But regardless of what was meant by the product, whether things or dollars, the factors of production surely had to be either physical facts or value facts. If the former, there was no way of finding proportions, since in a rigidly logical sense thousands of different kinds of events would contribute toward the joint result, it being impossible for us to find even for a given moment all the ratios really involved. And of course, the ratio would change virtually for each act of production, owing to a general law of change over which humans have no control. But furthermore, if physical things were meant by proportionality, who could say how much of each was used, or should be used, to achieve the best results, and what bearing this output of a physical supply had upon its value?

It may therefore be assumed that economists had in mind a principle of value proportions when they showed that there is but one best way of doing things. But in that case also the law was not as startling as might have appeared at first sight. For in the first place it remained, then, to identify definite amounts of values with definite amounts of things, of materials or services, if the law was to assume a technological significance and teach anything for future ends; but such a specification of things relative to their values was never attempted. And so in the second place the law necessitated a reduction of all physical

factors of production to four legal factors, the relative amounts of which could be varied to find out which proportion yielded the largest pecuniary values. From the entrepreneur's standpoint, of course, a ratio would then have to be established between product and profits to warrant his choosing a particular proportion; for it was net profits that interested him, not merely sales. And how could this relation be made part of the proportionality argument? But again, values of management and labor could not be compared with those of capital or land; nor could profits be regarded as causally derived from such proportions of values, since these in part presupposed a profit. Nor was there any way of tracing particular dollars of any one agent to particular physical supplies turned out, or of distinguishing nicely between dollars of labor and those of interest on capital.

The whole theorem therefore proved delusive. For all economists it meant that on two counts the only law deemed important in Production had proven worthless, viz., first because it established neither constant physical nor value ratios of specifiable elements of production, and secondly because it treated of elements that orthodox price and income students did not know by name, since they recognized only four legal factors. Thus we have on the one hand the familiar fact that a producer, by violating the law of physical proportions, could reduce supply and frequently swell his profits; and on the other the startling fact that *if* stable quantitative relations for highest efficiency were discoverable, they could mean nothing for a theory of distribution; and this in spite of a "productivity-theory" among Marginists.

On Definitions in Production.—Let us pass over now to

some basic terms that were usually stated preliminary to a discussion of production. (See Table III.)

Beginning with J. B. Say, the French popularizer of Adam Smith, definitions made up a notable part of economic science. More and more the drift was toward precision and logical interrelation. Agreement was by no means easily reached, nor permanent on all leading ideas; but none the less unanimity was attained more nearly on definitions than on laws of applications. The chief definitions sprang directly or indirectly from psychological and legal premises, or from ideas of law and social causation that will soon engage our attention. They reflected the longing for systematization, but they also brought in their wake certain anomalies that have elicited much good-natured ridicule on the part of critics.

Before production could be defined, or at least immediately after defining it, a few other terms needed to be known, and so bounds had to be set to economics from the start. Utility, it was decided, was anything capable of gratifying any want. It did not matter what the hygienic or moral or political consequences of an act of satisfaction, if this latter depended on the use of a certain thing, this thing was a utility, or was useful. This was a commonplace that, on the whole, found speedy acceptance. If, however, it was asked whether any utility constituted a value, or whether the creation of any utility was a productive act, two different answers were given. To some it seemed that utilities had to be scarce in order to become "economic," while others granted the significance of scarcity for economic studies, but did not absolutely insist on it as a prerequisite in production. The great majority leaned toward inclusion of scarcity, defining it as insuffi-

TABLE THREE

LOGICAL ORDER FOR BASIC CONCEPTS OF ECONOMICS AS A SCIENCE

Production	*Consumption*	*Price*	*Distribution*
Want			
Utility	Use	(Absolute) Value	
Scarcity			
Transferability		(Exchange) Value	National Dividend
Good (Value)		Money	
Wealth			
Production	*Consumption*	*Price*	*Distribution*
Factor (Agent)			Share
Land			Rent
Labor			Wages
Capital { Goods / Fund	Savings		Interest
Enterprise			Profits
Rates of Return	Utility, Degrees of	Cost (Expenses) { Ris'g / Fall'g	Cost (Expenses)
		Supply	Supply
	Complementary Utilities	Demand	Demand
			Impatience
		Capitalization	
			Productivity (Standard of Living)

NOTES. 1. Premises are: Private property, freedom of contract, of vocation, and of residence (i.e., a competitive régime).
2. For Marginal economics add the concept of margins of several kinds (see Tables One and Two).

ciency relative to demand at some price. Anything so abundant as to be had for the asking would not entail effort on the part of man. Nobody would give anything for it, and so it could not enter the markets. Since, then, neither effort nor exchange nor price would connect with "free" goods, they did not concern the student of economics. What was wanted was a thorough comprehension of price and income facts. These held out the greatest promises to a believer in social laws. And so insufficiency of supply became a natural attribute of things economic.

But it was not done with these two conditions. For human laws plainly affected economic organization and processes. The legal facts had even in the eighteenth century provided premises for economic investigators; and so it came about that legality, too, figured as a quality of value. If governments prohibited trade in a scarce utility, that made it theoretically non-economic. Transferability was as necessary a condition as usefulness or scarcity. In other words, to have value, a utility had to be both scarce and transferable; but having these two attributes it became a "good." It became valuable, or had value; and the creation of values was a productive act or more briefly, Production.

An article might, then, be transferable so far as the law had anything to say, but if not regularly in the market it was not an economic good. Neither were illegitimately exchanged articles economic in the strict sense, nor things vital to life or welfare, if too plentiful as a rule to fetch a price. On the other hand, values could come into existence without any effort by men, for if they were wanted, scarce, and legally exchangeable, like diamonds found accidentally, or appreciations of ground held by

speculators, this fact itself made them goods and an object of economic study. It was not labor that decided the case, as was shown from the inception of the science, but want (respectively demand) for goods at a price.

Private property and an individualistic viewpoint were responsible for these definitions. Wealth by common consent was an individualistic concept, although opinions as to its exact meaning differed. One writer said that wealth "consisted" of all *potentially* exchangable means of satisfying human needs"; another meant by it the "sources of human *welfare* which are *material*, transferable, and limited in quantity";[4] a third one thought it consisted of "material objects owned by human beings"; while Mill in his "Principles of Political Economy" maintained: "Everything forms a part of wealth which has a power of purchasing; for which anything useful or agreeable would be given in exchange".[5] Thus a variety of interpretations might easily be hunted up; but predominatingly wealth signified scarce, useful, transferable things, that is a fund of values rather than of utilities irrespective of their scarcity. But of course, since value was not necessarily a tangible utility, and since each person was his own judge as to what he wanted, values might pile up without benefit to the larger social group, without taking such form as the majority could perhaps wish. Thus a practical defect of the definition of production was its indifference to classes of concrete or inconcrete goods, to their ratios, and to a standard of value that might have made the sum of individual fortunes equal to the wealth of the nation. By degrees goods were classified

[4] Keynes, J. N., Scope and Method of Political Economy, 1896, p. 92.

[5] Preliminary Remarks.

variously, to serve new purposes; but that only emphasized the error of defining production as a creation of values. Thus goods were divided into reproducibles and non-reproducibles, into publicly or privately owned goods, into tangible (form) or intangible (time, place, and rights) values, into land and non-land wealth, into capital and consumption goods (capital embracing however three distinct categories, as already shown), into goods admitting of one use and durable goods; etc., etc.

Of recent years these classifications have won popularity and been put to excellent uses; but they could not offset the acquisitive, hedonistic foundation of the main definitions. Economics had developed into a science by dealing exclusively with exchange-facts, and this ideal was never abandoned by the bulk of economists. If the question came up: What is "economic?" they said perhaps: Data relating to men's activities in earning a living. But this was not true to their analysis of price: nor would it have permitted the definition of wealth as a fund of values. Neither free foods nor facts outside of exchange were included in the answer. What actuated economists most of all was the desire to reduce the manifold of economic life to uniformities and regularities, and to do this the psychology of sensationalism was invoked. Laws of association furnished the grounds for an "economic man". Through this abstraction a self-sufficient exchange-mechanism was constructed within which real laws should obtain, explaining adequately all wealth-data. This was the program adhered to by all groups except Historians and socialists. Because of these premises economics remained in so large a part a tissue of postulates and circular reasoning.

PART TWO

A CRITIQUE OF ECONOMIC METHODOLOGY

A RESTATEMENT OF THE PROBLEM

So far our concern has been with doctrinal economics, that is, with the main body of economic teachings and with the psychological premises underlying them. It has been shown that sensationalism is an untenable theory of valuation—something conceded by all. Furthermore, there can be no doubt that psychic states, strictly speaking, are always immeasurable, and thus preclude the use of psychology for measurements as exact as those that Utilitarianism or Marginism laid claim to. In short, not only did margins as a technical aid prove inadequate, but in addition the groundwork of orthodox economics gradually crumbled because of changes in sciences basic to the old discipline of catallactics. Price, income, and productivity laws are not what they seem to be. Real quantitative laws have not been found in those divisions of economics, although as a qualitative analysis sensationalism rendered excellent service as long as psychologists themselves could offer nothing better.

But the question now is: If a new principle of valuation, and in part even of human motivation, must be found, what becomes of statics and catallactics as principal features of the conventional economics? Is it possible to retain these ideas and terms, if the key to valuation, to income and economic history, must be sought in facts entirely at variance with those expounded by eighteenth century philosophers? Supposing we desire to continue

our economic researches for laws, must we not discard more than our old psychology, and will not many questions arise that are not directly economic?

In other words, with the abandonment of certain fundamental premises of orthodox economics we are forced also to face anew the query whether economics is a science as pictured, whether it may continue to be treated as a science providing generalizations comparable with those of physicists, whose labors were originally the inspiration of Quesnay and Smith. A methodological problem is involved because of the nature of our subject, and because the accepted methodology of the first formulators of catallactics has likewise suffered from changes in allied fields.

We may ask thus: If psychic quantities could not furnish the degree of empirical exactness once hoped for, shall we give up this type of research entirely, or is there another way of relating economic events quantitatively? Is our conception of a law to remain what logicians have until recently preferred it to be, or may we draw a comparison between physical "exact" law and others in such a way as to prove continuity between, say, physics and economics? What is the difference between law and correlations of the statistical sort? How far may laws be arrived at deductively, conformable to principles described by Formal Logic? Are physical events causally related in a sense that the socio-economic are not? Is causation something distinct from either law or correlation? Can we properly impute to particular events or groups of events a value that economists may use for distributive theories, or when bent on a moral errand? What is to be said in favor of a social science that agrees with our present-day information on human nature, on

the limits of inference, on the essence of knowledge and truth-finding?

Approached from this angle the defects of the Utilitarian-Marginal teachings become much more significant than appears at first sight, thus leading up to questions of method that are far from incidental in an appraisal of the outlook for economics. What must be examined evidently is the inward nature of deduction as used by scientists, the relation between deduction and induction in a formal analysis, and the possibility of arriving at any economic laws by a method purely deductive. It is a case for the methodologist as well as for the student of pricing processes. The relative merits of deduction and statistics are at stake on the one hand, and those of experimental versus statistical induction on the other hand. All in all, a wide survey must be made that goes far beyond the bounds of any one science. If sensationalism is wrong, points in orthodox logic also need correcting. If statics and catallactics are shown to disagree with our newer thoughts on human nature, on social processes, and on the laws imbedded in them, then new light may also be needed for defining the scope of economics, for discovering the general principles governing a delimitation of sciences, for tracing the right relation of economics to ethics, and of economics as a science to applications thereof by politicians or theorists of diverse tempers. Whether economics is a science we may not consider an important question in the end, but that new topics deserve our attention, that methods and ideals will call for recognition which earlier economists ignored, this change must assuredly excite our interest.

A critique of scope and method is therefore an integral

part of our work, especially after the old foundations have been pronounced unreliable. We must become methodologists before gaining new strength as economists. In methodology we unite the interests of logic, epistemology, and psychology. We turn to the facts of psychology for an insight into the ultimate questions of knowledge, but we must also link it with logic and scientific method. The methods of science in general have special applications for any one science, and the analysis of law and causation in general bears closely upon our view of truth in sociology or economics. As methodologists, in fine, we gather materials scientific and ultra-scientific or metaphysical. Yet this does not prevent us from keeping our eye steadily upon the main economic problem, nor from proceeding empirically at all turns. Methodology is always a natural terminal in scientific investigations, but particularly so now that we are confronted with new aspects, with data different from those that guided logicians a century ago.

CHAPTER SIX

INFERENCE

Deduction.—The subject of inference has been for many years studied under two headings, viz., first as deduction and secondly as induction. It was understood from the start that deductive reasoning is in a class by itself, but eventually thinkers also came to a realization of the importance of induction, and of its peculiarities which deserve serious consideration no less than the syllogism. Indeed, for purposes of social science it can hardly be stressed too much that the major question is not whether deduction and induction represent opposite types of thinking—although this has often been said and made a center of discussions—but rather what the difference of materials is that these two forms of inference work with, and to what extent conclusions from *chosen* premises may claim a superiority over the generalizations gained by induction. Certainly it is agreed that deduction is by no means the whole of the process by which men arrive at worth-while knowledge, nor need the methodologist busy himself with the many problems involved in formal deduction except in so far as they throw light upon their relation to scientific methods. For our present needs therefore the best plan is, first to point out once more the salient features of formal deduction, secondly to contrast with them the broader demands of scientific induction, and third to add a few words on the ultimate postulate under-

lying induction, not to cast doubt upon the validity of scientific conclusions known as law or causation, but to prepare indirectly the way for a close examination of the relative merits of inductions by natural and social sciences. In reality inference and law or causation are inextricably interwoven. There is scarcely an excuse for sundering them. But since method, and notably the process of reasoning, is after all distinct from its subject-matter and final results, a consideration of each by itself is justifiable.

As to deduction then: If we take the old syllogism about Socrates as a most important type, we find that everything depends upon a proper statement of relations between universals and classes. It is with concepts, with axioms and degrees of inclusiveness that formal logic builds. Laws of Thought are: *A* is not not-*A*; everything is either *A* or not-*A*; and *A* is *A*. With the aid of these and possibly a few other postulates such as that an assertion is either false or true, and that some propositions may be recognized as true, logicians have constructed their moods and figures of a categorical or disjunctive or hypothetical syllogism. In the premises appear a generalization and a particular instance, and in the conclusion the particular is connected with a universal according to fixed rules.

If we say, thus, that all men are mortal, and that Socrates is a man, we must infer that Socrates also is mortal. It is the definition of a syllogism that, two propositions being given, a third must necessarily follow. The relation of the particular to the universal is brought out in this manner. Terms and definitions are important in that either we are dealing with one attribute only, such

as mortal, and equate it with "all men," or else make the more inclusive term contain the less inclusive. That is, either we say: All man = mortal, Socrates = a man, therefore Socrates = mortal; or we make "all men" a part of "mortal," and Socrates a part of "all men." It then follows from the axiom "a part of a part is a part of the whole" that Socrates must die. But we might also find our justification for the conclusion in the other axiom that *A* being equal to *B*, and *B* equal to *C*, *A* must likewise be equal to *C*. So far as syllogism is concerned, this is the basis of its validity. As long as the human mind remains constituted as it is to-day, conclusions may be drawn from antecedents according to definite procedures known to Formal Logic. The terms need not represent any meaning, nor refer to facts of the outside world. It is a mechanism of classes that we are dealing with, a juxtaposition of magnitudes in a certain order. Whether we construct the syllogism for the first time, and thus arrive at a novel impression, or repeat the performance for the millionth time, the results will always be the same, and the proofs as good the last time as the first. "Discovery is an accident, and not an essential of inference"; [1] and "deduction is nothing more than inference from postulates, whose truth or falsity is immaterial to the argument." [2]

It should not surprise us, however, if in view of this abstraction essential to syllogistics men have asked whether discoveries can be made thereby, or whether not all formal deduction involves a real, albeit adroitly veiled, begging of the question? This criticism certainly

[1] Bosanquet, B., "Logic," 1888, vol. II, p. 8.
[2] Mercier, Ch., "A New Logic," p. 404. See also Lewis, C. I., "Survey of Symbolic Logic," 1918, pp. 359-60.

gained momentum with the Renaissance in Europe and eventually led to the study of scientific methods which now are part and parcel of an inductive logic. The conviction grew that deduction is not the fruitful source of information that it was heralded to be. Instead men came to believe—to quote a passage representative of others of a similar tenor—that "inference is over when the premises have been brought together," [3] it being "the peculiarity of the syllogism that the conclusion does not advance beyond the premises . . ." [4] Increasingly then logicians have stressed the difference between syllogistic premises and those serving us in everyday life or in the pursuit of a systematized body of knowledge. The question has been persistently put: How do we get hold of our universals in formal logic? What entitles us to construct an equation out of predicates if they are not simply abstract magnitudes or entities-in-order, but meaningful facts gathered as part of our life's experiences? Or to apply the problem to the proposition about Socrates, why do we assume that all men are mortal and that Socrates is a man? How do we know this? What rational grounds have we for the assertion?

Psychology of Induction.—Now so far as we are willing to discuss this matter in terms of psychology at all,[4a] we

[3] Schiller, F. C. S., "Formal Logic," 1912, p. 208.

[4] Bain, A., "Logic, Deductive and Inductive," edit. of 1874, p. 207. See also Jevons, W. S., "Principles of Science," 3. edit., p. 219. For a later critical discussion see Joseph, H. W. B., "Introduction to Logic," 1916, chs. 14, 17-8.

[4a] For literature on relation of psychology to logic see, among others, the following: Husserl, E., "Logische Untersuchungen," 1900, pp. 50-227, where a transcendental logic is defended on broad, Hegelian lines. For relation of logic to scientific method see *ibidem*, Part I, p. 23 ff.; Nelson, L., "Ueber das Sogenannte Erkenntnisproblem," 1908, in criticism of epistemology in general; Hegel, G. W. F., "Logic" (translated by Wallace, W., 1912), vol. II, p. 30,

can never hope to improve much on the diagnosis submitted by David Hume in the second quarter of the eighteenth century. It must always redound to the glory of this searching skeptic that he was the first to elucidate the processes of induction to mankind, laying down a few basic principles that will have our approval as long as human nature remains what it is. Hume to be sure was mainly interested in an old problem of causation and of the limits of our knowledge, and so went farther in his disillusioning exposition than we now hold necessary. Kant was not satisfied that man was as impotent as his predecessor had seemed to prove, nor has the world since then failed to appreciate the positive elements of science, the fundamental sense in which events are real and our actions progressively rational. Hume therefore should not be read as the final authority on reasoning, for this was not even his principal theme. But there is room for a psychological interpretation of logic, and notably of induction.

If then we ask why we believe that all men are mortal, disregarding for the moment the logical validity of our beliefs, the answer will be pretty much what the Scotch philosopher himself pointed out, although in details we may differ from him. We have to admit, in short, that

etc.; Gibson, W. R. B., "Problem of Logic," 1908, p. 104; Enriques, F., "Problems of Science" (translated by Mrs. K. Royce, 1914), pp. 47, 108, 121. The psychological basis or aspects of logic are emphasized by the following: Mill, J. S., "Logic," 1843; Pillsbury, W. B., "Psychology of Reasoning," 1910; Dewey, J., "How We Think," 1910; Schiller, F. C. S., "Formal Logic," 1912; James, W., "Principles of Psychology"; Goddard, H. H., "Psychology of the Normal and Abnormal," 1919, pp. 185-87; Jastrow, J., "Psychology of Conviction," 1918, Preface; Messer, A., "Empfindung und Denken," 1908, pp. 151-83; Ward, Jas., "Psychological Principles," 1920, p. 348 *et seq.* See also Lipps, Wundt, Sigwart, Meinong, Höfler, and other psychologists or logicians.

men are so constituted mentally as to be impressed with repetitions of events in part or in toto. Repetition almost literally turns out to be a source of truth, the past serving as a guide to the needs of the future. Recurrences and their similarities urge us to accept them as ways of nature, as bases for prediction, even though we are aware of the risks involved. What is more, we are able to-day to go beyond what Hume called the force of Custom, since physiologists have acquainted us with the neural mechanism through which excitations are guided and interconnected so as to ensure the right sort of responses in thought or deed. We need not take this picture of the neurologist too seriously, of course, for not even the essentials have been definitely verified; nor must we forget the contributions of other investigators in the field who would interpret consciousness as a phase of metabolism in all its diverse manifestations. However, as part of a psychology of inference the following facts deserve attention.

Thus we know that the neurons which are the ultimate vital units of our nervous system, possess among other properties those of conductivity, plasticity, and retentiveness in a high degree. They are so interconnected as to permit a recording of, and a response to, excitations either directly, as in the reflex-arc, or indirectly with the aid of the lower or higher brain centers. In these three planes our adjustment processes move, and as we progress from animal to human life the by-product of reaction takes more and more the form of an artificial environment, whose rôle gains steadily on that of the physical environment.

Nerve-paths are partly set at birth, but partly also laid down during our lifetime. With the help of associative neural units and the areas of connection distinguishable

in the cortex a multiplicity of paths and responses is assured. One stimulus may end in several reactions, and one reaction may wait on several stimulations. Innumerable ramifications are being built up, which obedient to laws of selection, inhibition, and association, provide eventually a ready apparatus for meeting the exigencies of daily life. The learning process being one of adaptation to immediate and mediate surroundings, and memory being ever creative in the molding of our perceptions and ideas, man appears as the most rational of beings, who dwarfs his heritage of instincts by the enormous cumulations of knowledge acquired postnatally.

But to follow up our argument. The resistance existing at birth at the points of contact of the neurons is gradually broken down, so that our responses to stimuli become easier in proportion as they repeat themselves. Successive adjustments gain in accuracy and completeness. Practice makes perfect, and habits arise as a "fixed form of reaction." We get used to things, and attune ourselves, so to say, to the prevailing pitch of experiences. Thus, as we learn to respond and be ready at short notice, we also cultivate unwittingly a mood of expectation with regard to events that do not demand direct response. The oftener an event recurs, and the more regular its outward make-up, the more likely we are (barring counter-irritants that "inhibit") to look forward to a further repetition of such events. A belief is engendered in us that such experiences will be met with again. We count on them whether liking or disliking them. We anticipate them, even though occasionally fooled. We expect events to recur, basing our faith on nothing but the great number of happenings in the past. Thus faith is born and bred in us; thus beliefs

spring up not merely as regards common places, but notably too with respect to recurrences that have always been purely mental, given to us by our elders as a part of our social heredity. Thus reason has little to do with our expectations, and habit everything. "So far as empirical science can tell us anything about the matter, most of the proximate causes of belief, and all its ultimate causes, are non-rational in their character." [5]

What is more, it should be emphasized that the force of enumeration or repetition applies to groups of events fully as much as to individual ones; or to state the matter more precisely, we are led to believe the recurrence of groups of events even when only part of the group recurs at the moment of our speculation. All experience comes to us in series or blocks of events, whether they be concrete things or psychic data or such events as the world around us offers in countless numbers. Strictly individual facts do not exist for us, though we have the ability to abstract them, and for specific purposes may attach to them distinct individual or differential meanings. And this circumstance that we sense everything in groups has had its share in developing in us the physiological apparatus for recording and remembering the groups. We learn by association, and connect events just as they appeared to our senses. "Acquired mental connections" of this kind account for our thinking of objects not present, and show why sensations should quite early in life be transformed into perceptions that differ from the former as much as a chemical compound differs from the elements out of which it arose. Whatever the final explanation of the

[5] Balfour, A. J., "Foundations of Belief," 1906, p. 339. See also: Hibben, J. G., "Inductive Logic," p. 36; Jodl, F., "Lehrbuch der Psychologie," edit. 1916, vol. II, pp. 342-43.

synthetic powers of our brain or mind be, this creative element has a physiological parallel in the associative capacities of the nervous mechanism. Events often found together, or having certain features in common, are likely to be associated in an act of recall. "Whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (of course, in imaginal terms) all those sensory and imaginal processes which occurred together with it in any earlier conscious present." [6] Either through resemblance or through contiguity the association is brought about, and the *latter* is the one here applicable. For if events have frequently and with much regularity happened together, either in succession or simultaneously, we shall expect them to recur again as a whole. Though only a part of the situation be present to our senses, the principle of association restores to our mind the whole of it. We think of what is not present at the time, but was present and contiguous in time or space with what we do perceive. Thus our expectation is not only one of recurrence in the future for any one event, but it likewise relates to larger sets of events, such as a thunderstorm or a landscape or street-scenes, or whatever occurs to us. "To have a clear case of expectation it is not necessary that we should distinctly remember any previous experience like that expected, but only that we should have actually present some earlier member of a series that has become firmly associated through previous experiences." [7]

[6] Titchener, E. B., "Textbook of Psychology," 1910, pp. 378, 393-95. For eighteenth century view of Logic see, e.g., Hartley, D., "Observations on Man," 1740, vol. I, pp. 359-60; Brown, Th., "Lectures on Philosophy of the Human Mind," edit. 1830, Lecture 49.

[7] Ward, Jas., "Psychological Principles," p. 209. On the relation of habituation to the causal concept see also Russel, B., "Our Knowledge of the External World," 1914, pp. 222-23.

Besides, it is brought out by our experiences that whole *correlations or groups of events may be predicted the more safely, the larger the percentage of parts perceived at the moment, and of course the more regular the recurrence in toto up to date.* Thus, if at a given moment I perceive *A*, *B*, and *C* out of an ensemble of *A*, *B*, *C*, *D*, *E*, *F*, and *G* I shall be more confident that *D*, *E*, *F*, and *G* will also appear immediately afterwards, or *are* there without my stopping to itemize them, than if I perceived only *A* and *B*. The chances for error have proven greater in an expectation resting on a slender basis of facts than on a broad basis, and so our inference goes back once more to an induction from numbers. Whatever the tests urged by science, unguided inference follows this criterion and applies it to groups of events no less than to individuals.

But though this explains—as far as such facts can—why we consider all men past and future to be mortal, it does not tell us why some one being should be classed among men, why Socrates in our syllogism should be spoken of as a man. It still remains to find out on what grounds we place him in the class, and how much evidence we have gathered to justify this step.

Now, here again Hume led the way for all later investigators. He pointed out how largely we are governed by appearance, notably by differences and resemblances,[8] and how the principle of association operates so that a reappearance of a few similarities prompts us to expect the

[8] For a recent opinion on modes of association see: Titchener, E. B., "Textbook of Psychology," p. 376; Warren, H. C., "Human Psychology," 1920, ch. 16; Calkins, M. W., "A First Book in Psychology," 4. edit., pp. 117-24; Hunter, W. S., "General Psychology," 1919, p. 287; Jodl, F., "Lehrbuch der Psychologie," 1916, vol. II, pp. 147-61. For explanation of association on physiological grounds see also Angell, J. R., "Introduction to Psychology," 1918, p. 165, and Muensterberg, H., "Psychology," 1914, pp. 111-19.

others that on a previous occasion were coupled with them. Continually we are in touch with identities amid diversity. Though nothing may, on close inspection, be exactly like anything else, yet certain outstanding resemblances become noticeable, and these first catch our eye and lead to conclusions as to a relation between the old and the new. Though we may at times be proven wrong, in the long run there is reason for our habits. We note sequences of events, and are satisfied that upon certain links others will follow because they followed before. We equate unequals, so far as our knowledge at the time goes. We go by circumstantial evidence and take risks in predicting the future from the past. It is not, therefore, that things discrete in time or space are completely identified, by an enumeration of all the characteristics known to us, but that we select, in the first place, a few from among a large number of attributes, and in the second place do not consider even all of these necessary for classing things or events or men, as in the premise of our syllogism. Most of our everyday reasoning revolves about such fragments of evidence. In nine out of ten cases it consists, to quote from W. James, of "the substitution of parts and their implications or consequences for wholes." [9] "There are two great points in reasoning: First, an extracted character is taken as equivalent to the entire datum from which it comes; and secondly, the character thus taken suggests a certain consequence more obviously than it was suggested by the total datum as it originally came." [10]

[9] "Principles of Psychology," vol. II, p. 330, edit. of 1893. An early suggestion of the idea may be found in Locke's "Essay Concerning the Human Understanding," 1690, Book 4, ch. 2, §§ 1-2.

[10] James, "Principles of Psychology," p. 340, and pp. 645-46. See also: Goddard, H. H., "Psychology of Normal and Abnormal," p.

Thus abstraction expedites comparison and evaluation. Qualities stand for like qualities plus others that are inferred, because experience has again and again shown them to be coexistents. The larger the percentage of common elements shared by two situations or groups of events, the greater the likelihood of their being completely alike. This is the lesson taught us in our life, and because of it inference leads to reliable predictions. (Still, if it does prove false, that changes in no wise the principle of inference itself.) Much of our thinking revolves about such considerations of similarities. It forms part of our musings and deliberations on practical affairs. "Reflection," as has been stated by an American authority, "implies that something is believed in (or disbelieved in), not on its own direct account, but through something else which stands as witness, evidence, proof, voucher, warrant; that is, as ground of belief." [11] Or to quote from an English critic of formal logic: "If analogical argument is not 'formally valid,' no argument can be 'formally valid.' For every argument, whether 'inductive' or 'deductive,' is really analogical." [12] Ordinary acts of recognition consist of such inferences of identities as are not at the time established. We perceive more than our eyes see. We add to what is presented in the shape of physical stimuli. We recognize friends by a few signs, a relatively large number of other facts that we should need for identification being altogether ignored. Change the

192; and Jodl, F., "Lehrb. der Psychologie," vol. II, pp. 348-52. For objections to this view see Bosanquet, B., "Logic," 1888, vol. II, p. 58.

[11] Dewey, J., "How We Think," p. 8.

[12] Schiller, F. C. S., "Formal Logic," p. 342; Pillsbury, W. B., "Psychology of Reasoning," pp. 230-37; and similarly, Joseph, H. W. B., "Introduction to Logic," ch. 24.

clothes or the beard of the man, and you may fail to know him! Make sure of a few characteristics, and you have the whole man! Socrates was to his contemporaries a man because of a few outstanding traits out of the total that constitute the species.

Relation of Induction to Deduction.—Bringing these principles of reasoning to bear upon our questions of economic methodology, we may well subscribe to the words: "The apparent paradox is that in order to have facts we must depend upon inference, while inference in turn rests upon facts." [18] That is, the two main divisions of logic or inference are inseparable, though a distinction between them is absolutely necessary. Deduction and induction cannot be torn apart. Men have always recognized this fact.[18a] In one sense, to be sure, the syllogism is the prototype of all reasoning, but as regards its premises they force us to consider the principles of enumeration and substitution by analogy which may be employed without the kind of substitutions practiced by formal logic. Theoretically induction is not concerned with the laws of thought enunciated by the logician, nor need this latter interest himself in the origin of his propositions from which he draws a certain conclusion. But practically all our reasoning combines enumeration with analogy, and both with an explanation of the particular through a general, through a universal in the logical sense.

Deduction in Science.—All this is a commonplace and calls for no further elucidations. We must however realize, in the second place, that precisely because of the

[18] Bode, B. H., "An Outline of Logic," 1910, p. 198.

[18a] For statements to this effect see: Wundt, W., "Logik," vol. II, Part I, § 1; Joseph, "Introduction to Logic," ch. 18; and Hunter, W. S., "General Psychology," p. 340.

basic concepts of formal deduction it cannot be a fit method for sciences dealing with facts. To call a given science deductive is to give rise to misconceptions, to challenge criticism, unless we wish to contrast merely two types of scientific research, viz., experimentation or statistics on the one hand, and introspection or reflection on the other. It is admissible to call mathematics a discipline of deduction in a true sense of the word, because no empirical data are involved in its reasoning, because everything is derived from some premises agreed to beforehand. Definitions are postulates, and the so-called axioms have also proven to be postulates for the largest part. Thus the mathematician uses deduction pure and simple, relying upon reflection or upon experimental data only when they may prompt his intuitions. But all pursuits other than mathematics must choose their premises carefully if their conclusions are to have any value. They cannot abstract a few facts from an actual world, and then erect upon them an imposing edifice of generalizations. Their reasoning may be good, but their starting-point will almost surely prove fatal. The more complex the data to be reckoned with, the less reliance may be placed upon a few premises however well selected, and the more clearly the science must be inductive. Especially must the social sciences for this reason be chiefly inductive. Not but that a deductive form of reasoning may be employed throughout the work. Assuredly so! But this kind of deduction in form will be united with induction, and with a periodic revision of premises used for any particular argument. The logician is intent upon proving things —and everything may be proven with a judicious choice of assumptions! But a scientist is most eager for veri-

fications, for establishing a truth squaring with facts, with evidence of the senses wherever it may be forthcoming. Thus the alleged deductive social sciences will not mean what writers have tempted us to believe. If economics, for instance, is a deductive science, then it will not be such because of its arguing by means of a syllogism—for that is true of all sciences—but because its laws or correlations are not derived from experimental or perhaps statistical tests. What in addition to this distinguishes economics from, say, physics, is a second question. But so much many be said à propos of deduction.

Logical Validity of Induction.—Finally, as to the old problem of the validity of induction as a means for discovering verities, we must agree with the traditional refuge in a law of the uniformity of nature. There is no doubt of the logical need of some such assumption if enumeration with or without substitution by analogy is to yield general conclusions. A mechanistic view of the cosmos is probably the most suitable for the purposes of the argument from induction. We must grant at the outset that there is fixity and finiteness in nature. We must formulate a principle of necessary connection regardless of the irregularities presented to our senses in the interrelations of perceptual data. We must say: "There are such invariable relations between different events at the same time, or at different times, that given the state of the whole universe throughout any finite time, however short, every previous and subsequent event can theoretically be determined as a function of the given events during that time." [14] Or we may take a less dogmatic attitude by siding with the writer of the following: "The

[14] Russell, B., "Scientific Method in Philosophy," p. 221.

law of the Uniformity of Nature appears to me to amount to an assertion that an analogy which is perfect, except that mere differences of position in time and place are treated as irrelevant, is a valid basis for a generalization, two total causes being regarded as the same if they differ only in their position in time or space." [15] In the end, however, we shall be driven to a still more skeptical pose, preparing thereby a basis of comparison between experimental and statistical induction, although it may not suggest this at once. Namely we must grant in all candor that "unless inductive conclusions [of any kind!] be expressed in terms of *probability*, all inductive inference involves a formal fallacy," [16] to wit a *petitio principii*. "An inductive argument affirms, not that a certain matter of fact *is* so, but that relative to certain evidence there is a probability in its favor. The validity of the induction, relative to the original evidence, is not upset, therefore, if as a fact the truth turns out to be otherwise." [17] Inference as such may be good irrespective of future proofs to the contrary, but we should not forget that "while it is often convenient to speak of propositions as certain or probable, this expresses strictly a relationship in which they stand to a corpus of knowledge, actual or hypothetical, and not a characteristic of the propositions in themselves." [18] Such is the lesson taught us by the whole of inference. Barring a total disregard of empirical data we are always liable to be in error, no matter how

[15] Keynes, J. M., "Treatise on Probability," 1921, pp. 226, 258, 264; Joseph, "Logic," ch. 19; Mill, J. S., "Logic," Book III, ch. 3, which contains a much quoted view on the empirical origin of the idea of a Uniformity of Nature.

[16] Broad, C. D., in *Mind*, 1918, p. 26. See also *ibidem*, January, 1920.

[17] Keynes, "Treatise on Probability," p. 221.

[18] *Ibidem*, p. 407.

well we reason. And as for differentiating sharply between real truths and invalid inductions in science, it is impossible to justify it in the light of the logic of inference itself.

CHAPTER SEVEN

LAW AND CAUSATION

Questions.—The fruit of induction is a generalization known as a law of nature. It is for the sake of reducing the multiplicity of data passing before his eyes to a relatively few fundamental laws that a scientist goes to so much trouble. Science has not accomplished all if it does not yield laws, if it does not subsume one set of laws under another of a still wider prevalence or of a more general form. Hence investigators in all fields may, to a large extent, be judged by what kind of laws they bring to view, this criterion being as valid for social as for natural sciences. But it will appear, as we go on, that there are laws of two very different types, namely, laws in the narrow sense and laws more loosely constructed and passing by the name of correlation. The question thus is: Should the latter be classed with the former? Is there a difference between them so great that they are virtually incomparable? Is it true that causation is part of a law of nature, but not of correlations when found in non-physical data? Or can we in some way establish a bridge between law and correlation, eliminating the causal aspect or robbing it of its portentous significance such as tradition has assigned to it? In the answer to these questions will lie a not inconsiderable part of our opinion about the scope and method, the character and possible goal of economics.

What is a Law of Nature.—Now, first of all, a law of nature is a statement of a regular recurrence of sequences or coexistences, or is this regularity itself. A sequence consists of things or events happening in succession within some time-unit, whatever it turn out to be on measuring it. A coexistence is a group of things or events existing together for definite or indefinite periods. The frequency with which these uniformities occur or recur does not matter. The only decisive trait is the absolute regularity of recurrence, as inferred inductively according to the canons of reasoning. Coexistences may or may not be regarded as a real class of laws. It depends, and authorities have differed on the subject. But such bundles as the qualities of the chemical element gold, or the items making up a living organism at a certain moment of time, enduring perhaps for a long span of time, such simultaneities have been called coexistence-laws, with and without due consideration of the causal values involved.

In the second place, a law of nature comprises regularities of not only individual things or events, *A* invariably occurring before or after *B*, but more especially regularities of sets of events such that *A*, *B*, *C* is linked inseparably with *D*, *E*, *F*. It is this interdependence of a number of distinguishable events that is most characteristic of our scientific laws. We are nearly always dealing with bundles of events, and not simply with individual units. However, it is a commonplace that science is interested in relations rather than in things, and that these laws of nature have in most cases a quantitative meaning as well as a qualitative one. That is to say, we not only establish a uniformity of data which because of

our senses must assume a quality aspect, but we are even more intent upon finding the relative amounts of these qualities entering into a uniformity. To say that a certain number of things, e. g., makes up a thunderstorm and that the law consists of the regular recurrence of these magnitudes, is one thing; but to ascertain the quantities of each element enumerated is still another thing. Both of these relations, or only one of them, may be essential to our idea of the law. Yet the fact of our seeking precise quantities is so familiar to all, that science is not inappropriately called the study and measurement of relations of magnitude.

The first law of electro-statics which says that "any two different substances brought into contact become electrified," treats of a correlation of events as such, meaning of things or qualities; and so notably also many laws of coexistence. But for the most part science is a treasure of quantitative relations with regard to such events qualitatively sensed. It seeks to measure these quantities exactly, to note the changes going on in one magnitude or another, in one group as against another. The quantitative relative changes are watched and recorded. The proportions of relative rates of change are fitted into a temporal and spatial order. Thus a law of nature becomes a statement of magnitudes fully as much as one of things or events. Thus chemists recognize combinations of elements previously defined, emphasizing phases of metamorphosis and measuring precisely the proportions involved in a compound. The law of combining weights for instance states that "in every compound substance the proportion by weight of each element may be expressed by a fixed number, a different one for each element, or by a multiple of

this number by some integer." Boyle's law of gases deals with relative amounts of gas and temperature, the constants for the changes being the burden of the theorem. Indeed, Boyle's law *is* this regular recurrence of variations put into exact quantitative terms for temperature, pressure, and the volume of a gas, these observed regular changes in the past serving as the basis of a prediction for the future.

In the third place, natural science tries to reduce its events to the greatest possible degree of simplicity. At last analysis the things or qualities of science are not really qualities from the common-sense standpoint, but they become qualities and are connected with perceivable common-sense qualities because of the manner in which all our knowledge must be garnered. To science the irreducibles, are,—let us say—atoms or electrons, or mere lines of motion or force; and so on. It is on account of this reduction of phenomena experimentally treated to nondescript, colorless, indivisible units that science is able to draw up formulæ of such definiteness and sweep. But if we look at these formulæ in our texts and treatises we shall nevertheless find that they presuppose the reality of objects constructed out of these units. That is, the laws involve a study of things precisely as known to our ten senses. What the reduction to last units does is mainly to point out similarities which a superficial examination would not reveal. We are told that " a body immersed in a fluid is buoyed up by a force equal to the weight of the fluid displaced," and this is a reference to common-sense facts, as well as to a relation purely scientific. We read again: "The resistance to the flow of an electric current varies directly as the length and inversely as the area

of cross section of the conductor." That also embraces two kinds of things or events or units taken from the world of percepts and that of concepts respectively. Heat, light, and motion in this way become identical facts for science, although to the man on the street they represent quite different items.

But let us put the matter yet differently.

The units of natural science comprise in most, though not in all, cases a rather limited number of things or kinds of events. These events become antecedents and consequents in the endless, ever recurring successions that constitute the warp and woof of our experience, but are skillfully culled out from the totality of relations by our professional student. We find procedures adopted by him, and distinctions made, that have no place whatever in the thinking of the untrained mind. The common-sense attitude is unsuspecting and optimistic, while for the scientist many difficulties intervene that may obstruct his view. A few crudely gauged unities exist for the former; many nicely weighed unities for the latter. If we take for instance such a familiar happening as a thunderstorm, and ask what divides science from common sense, we shall get our answer in two types of analysis of one and the same phenomenon. The average observer will point to wind, rain, clouds, lightning, and thunder as the salient features in the process. These things are most readily sensed by him, and so he enumerates them as *the* ingredients of a storm, declaring them to be its explanation. Other items, to be sure, like dust, flood, fire, etc., may be mentioned on second thought, but they will probably be granted not to belong really to the subject under investigation.

Now by contrast, how does science acquit itself of its task in studying this same set of events?

It becomes evident at once that factors will loom up which we would not offhand think of; that units are at stake entirely distinct from those perceivable entities known to common sense; and that order in time, or subsumption in a classificatory series, is insisted upon regardless of impressions received by our retina or tympanum. The whole phenomenon, which we shall here call an *event-complex* to distinguish it from the last, irreducible units of science, is broken up into four principal divisions; or at any rate we should find such a division convenient if we were to trace all its links from start to finish. But suppose we content ourselves with only one division, since otherwise we should have to go too far afield. Suppose we try to learn from the physicist's account of the wind alone what interests him most, and why his units for qualitative and quantitative comparison differ in essence from those of an untrained man.

We observe then that the scientist tells us of the weight of the atmosphere which is a result of the mutual attraction between the sun and the earth, deriving his notion of heaviness from this attraction and the fact of a mass of some substance. Science would also mention ether as a medium in which light travels, as well as the radiation of light from the sun to us. These rays, we should further be informed, are partly stored up by the earth, and partly reflected back, heating the superficies of the earth and the atmosphere nearest to it up to a certain altitude. Hence there would follow greater molecular motion of the air, this latter expanding and thereby reducing its mass per unit volume.

Being made lighter in this fashion the air will rise according to a principle of buoyancy, while the heavier layers sink to the ground. A series of currents are thus set up conformable to laws of convection, the currents growing the stronger the more marked the difference in temperature for the diverse strata of the atmosphere. Yet the air need not rise vertically; for a number of interferences are usually operating to which scientists pay some attention. The rotation of the earth about its axis for instance would play its part. Irregularities in the contour of the land, and different rates of absorption and radiation of heat for land and water bodies would likewise prevent a straight upward movement of air streams. And so other factors still. Only after due allowance for all of these elements could the wind beating on us during an electric storm be said to have been explained in a true scientific sense. The qualitative analysis would be approximately as indicated, and a quantitative would have to be made if the storm were to be accounted for per particular area or stretch of time to distinguish it from similar affairs in the past. But this last quantitative analysis would indeed be out of the question.

What is more, and in the fourth place, it is plain that the units of science are without exception intertwined with other events or units that do not for the moment form a part of our survey. All laws of nature are abstractions in that they refer to relations lifted carefully out of a larger whole. We may be mindful of the enveloping phenomena, but call them conditions at the time. We say that circumstances alter cases and that conditions are modifying factors affecting the qualities or magnitudes involved in our law. These accompani-

ments therefore, since they invariably surround the relations known as laws of nature, must be studied whenever a practical purpose guides us, whenever a special instance is to be "explained" in the light of broad, underlying principles which themselves are laws of nature. The conditions are perceptual adjuncts of a conceptual play, as it were. They are in their turn the parts of laws which we may or may not know. They cannot do otherwise than obey laws, for such is our understanding of the cosmos, such is the trend of our thinking. It may be impossible for us to measure accurately the effect of these conditioning elements, though as a rule natural scientists can relate them quantitatively as well as qualitatively to their subject under review. We have amassed a vast fund of just that class of facts treated as conditioning phenomena. But in meteorology, e. g., or in biology, or even in the throwing of dice where the turn-up of pips might be studied with a view to a law, our conditions defy nice calculation. We simply point out the fact that the conditions do not invalidate our reasoning, or the law proclaimed. We make allowances for such special cases and try to show why these members must be part of another law, or may some day prove to be one. We state laws of sound, for example, and consider the properties of the atmosphere in which it travels before explaining one particular rate of transmission. We learn to realize that water boils at different temperatures at different elevations; why freezing points may vary; why water will rise in a pump apparently contrary to gravity; why a feather will not fall as fast as a stone; and so on. Also, we give different names to one and the same law or set of facts incorporated in it, according as other events belong-

ing to other laws accompany it. Thus we call rust, metabolism, and fire examples of combustion because a union of oxygen with some other substance is involved in all three, and because this is the definition of combustion that has most recommended itself to us on scientific principles. Or we call a set of happenings an electric current in one case, and northern lights in another, or lightning in a third. And of course we may use our knowledge of these modifying elements industrially or otherwise, adding and subtracting till our artificial product seems to have little in common with the sequences or coexistences found in a state of nature. Thus we may make flashless or noiseless powders, produce light without heat-giving properties, and so on. The qualifying circumstances of a law of nature are therefore no disadvantage, nor could we imagine them away, when isolating our set of law-elements. What we must remember only is the consistency of these conditions with our basic concept of a law of nature. Rightly understood there are no exceptions to our law. Science so decrees it. "There are no breaches of scientific law; or of a law of nature. If events are observed which do not conform to what we have hitherto called a law, we conclude, not that the law is broken, but that we were ignorant of the law." [1]

Subjective Basis of Science.—From this follows, in the fifth place, that a law of science is after all a rather subjective affair; that it has a human no less than a physical side, and that we should never lose sight of this dual nature of our understanding if we wish to bring the several fields of inquiry into relation with one another.

[1] Joseph, H. W. B., "Introduction to Logic," 1916, p. 2. For a modified view see Whitehead, A. N., "Inquiry Concerning Principles of Natural Knowledge," p. 87.

Even scientists themselves have increasingly admitted the conceptual basis of their data perceptually derived, or perhaps rather: They have learned to make the concepts of a creative mind a terminus as well as the starting-point of their researches in sensations. Realism, to be sure, is the first postulate of all science. A scientist cannot afford to doubt the reality of the world about him, to question whether he exists or not, whether he may know things or not, whether what seems to be space, time, substance and change is such or not. To a large degree science is necessarily naïve, sharing this characteristic with the bulk of practical-minded people. Realism is a prerequisite to men of science at the outset. And yet they may feel free to dwell on their limitations as knowers, falling in line with philosophers who have at all times stressed the problem of metaphysics and epistemology. Thus the nineteenth century gave rise to a large literature deprecating the efforts of science. Laws of nature were viewed as products of a mind operating independently, to a certain extent, of the material realm around it. Phenomenalism made headway and rendered useful services. A skeptical attitude was fostered and made the grounds of a methodology, mathematics, and theory of knowledge that is metaphysical in spite of being championed by eminent scientists. Thus we might quote as follows: "Law in the scientific sense is essentially a product of the human mind, and has no meaning apart from men." [2] Science is "an uninterrupted, but progressive series of mental constructions, which series gives us an approximate idea (representation) of the interconnected system of Reality." [3] It would not be absurd "to attribute the whole

[2] Pearson, K., "Grammar of Science," edit. of 1900, pp. 87 and 113.
[3] Enriques, F., "Problems of Science" (transl. by K. Royce, 1914), Bart: "Problems of Logic," § 11 and § 19.

responsibility for the laws of mechanics and of gravitation to the mind," denying altogether that "the external world has any share in them." [4] The metaphysical creeds back of these three statements could not be called the same, but the spirit of phenomenalism is quite apparent.

However, it is no more necessary to espouse phenomenalism because we accept dualism in metaphysics or epistemology, than there is ground for one's being either a Kantian transcendentalist or a monistic materialist. These questions of choice, and points of refinements brought up by the professional speculators need not detain us. But plainly a problem of valuation exists. There is reason for a committal on the query whether law, respectively causation, are undoubted facts in the outside world, or purely our invention, or perhaps a third something. And there is likewise reason, from the viewpoint of the economist, for taking this last suggested middle road, for siding with the critical realists rather than with out-and-out idealists or uncompromising monists.

What seems necessary first is a resort to transcendence, if knowledge (including scientific data) is to become explicable without straining our demand for logic; and what again seems evident is the difference, not between a Real and a Knowable as Kant had insisted upon it, but between an object and its content for us, or between magnitudes of data and their relations. Granting that knower and the known are two facts, and granting also that an element of relativity enters into all our understanding of events, we are still able to harmonize the largest possible variety of data and problems in the one belief that the

[4] Eddington, A. S., in "Mind," 1920, p. 155. See also Bain, A., "Logic," 1874, p. 353.

objective minimum and our subjective maximum of experience are fused by a process which presupposes transcendence. More than sensation and science is at stake; but in them the raw-materials are given out of which we build our castles and creeds. If we may accept the words of a recent work on this subject, we believe "that 'physical' things exist independently of being known; that they may be our objects, but that they are never our mental content; that they differ in some respects from the quality-groups of our perception (e. g., in not possessing the secondary qualities which we find in our percepts); but that they stand in such causal relation to our percepts that it is possible for science to investigate some of these relations and some of the relations between the physical things, and thus to gain trustworthy knowledge concerning the laws of their actions." [5]

It is with our knowing process and our valuations, in other words, as with the act of perception that psychologists have dwelt on so often, and most illuminatingly. We see more and less than is presented to our eyes, and we see different things according to our angle of vision, antecedents of thought, etc. In the blend of points, lines and shades, for instance, which a book on psychology calls our attention to, we may see either a duck's bill or a rabbit's head, according to focus and predisposition. Both pictures may be read into, or out of, the data presented, but it is hardly possible to predict which one we shall first think of. There are principles that decide our interpretation, and it is not difficult to explain why and how both

[5] "Essays in Critical Realism" by Drake, D., Lovejoy, A. O., Pratt, P., Rogers, A. K., Santayana, G., Sellars, R. W., and Strong, C. A., 1920. See also essay by Pratt, p. 109, and Sellars, R. W., in "Mind," 1919, pp. 410, and 407-26.

readings are brought about; but there is no sure way of excluding entirely either one of them.

Thus life itself is a maze of potential values, of possibilities that alternate or succeed each other or are combined for some reason. A vast array of puzzle pictures, in one sense, is this Manifold! A meshwork so finely wrought and intricate, so susceptible to change, and so replete with promises and perplexities, that no one generation of students may hope to understand all, or to speak the truth for times unending. Or to state the matter more specifically, and in terms of the philosopher: "The sense of the outer existence of these essences [of reality] is indistinguishably fused with their appearance. But these two aspects of perception, the appearance of the character-complex and the (implicit) affirmation of its outer existence, must *in reflection* be distinguished." [6] More than that, "a law of nature—is not a uniformity which must be obeyed by all objects, but merely a uniformity which is as a matter of fact obeyed by those objects which have come beneath our observation." [7] "Before a rigorous logical scrutiny the Reign of Law will prove to be an unverified hypothesis . . . , and the certainty of our scientific inferences to a great extent a delusion." [8] "A 'law' is not an absolute self-evident certainty to be imposed on reality by main force. It is a flexible formula for application to cases, and gets its real meaning from the cases to which it has been successfully applied." [9]

[6] *Ibidem,* essay by Drake, D., pp. 20 and 24. See also Royce, J., "The World and the Individual," 2. Series of Lectures, p. 159. Also: Windelband, W., "Logic, in Encyclopedia of Philosophical Sciences," 1913, vol. I, p. 47.

[7] Jevons, W. S., "Principles of Science," 3. edit., pp. 737-38.

[8] *Ibidem,* Preface.

[9] Schiller, F. C. S., "Formal Logic," p. 320. Similarly, Mach, E., "Erkenntnis und Irrtum," p. 449.

Correlations.—Estimates like these should in themselves warn us not to be too dogmatic in distinguishing between science and common sense, or on the other hand between the laws of natural science and those other formulæ known as statistical correlations. We may say of laws in the narrow sense that they are (a) built up of what for the time are proven to be irreducible units—perhaps literally "atoms" —, (b) that these sets of units are properly detached from our perceptual sphere of experience, (c) that the conditioning factors are as a rule measurable and logically parts of other laws, and again (d) that these groups of events recur with absolute fidelity, constituting an interdependence that knows no exception. We may define our law of nature in this manner, but this still leaves a basis for comparing it with correlations of a more flexible sort.

But let us see what exactly are the differences and resemblances.

We find, to begin with, that there are classes of regularities not nearly as perfect as those of physics or chemistry, and yet valid for a given region or period of time. Even in natural science correlations are sometimes the best thing that research can unearth. Even in biology and for the meteorologist functional relations of an indeterminate number of variables play a prominent rôle. But as soon as we pass from the lower forms of life to the higher, and especially as we enter upon the domain of human happenings, the intricacy of relations becomes proverbial. What characterizes statistical relations therefore is the large and more or less indefinite number of units comprised in them, the variability of our qualities or events as well as of their respective magni-

tudes. We are studying event-complexes rather than ultimate units of matter or force. That is, we bring into quantitative relation such events as are directly presented to our senses, or as are supposed by natural science to be built up of smaller units of a homogeneous physical sort. Not always of course, but predominantly this is a fact. With life and growth-force comes complexity, instability, and an intermingling of elements that physicists and chemists know nothing of. The mechanical equivalent in one sense still rules, but for purposes of generalization it is dead or insignificant. We must regard our data as elements in a propositional function, to use the terms of the mathematician. We must accept units for measurement which are scattered over large areas, over long stretches of time, or whose composition is of vital entities, of cells and whole organism or their parts, of groups of beings studied in their relation with physical or psychic facts. Social sciences most obviously are affected by this peculiarity of the units from which the event-complexes are constructed, and whose several interdependencies numerically expressed are the essence of a scientific correlation. It is undoubtedly true that any change in the physical world may bear upon the so-called psychic facts under investigation. An extermination of noxious plants or insects, alterations in the income of individuals, or in methods of production and exchange, or in types of association of men, or in the uses of things, and in personal valuation and institutional policy—these and other changes mean a realignment of elements in every correlation we may aim at or have already discovered. In other words, owing to the dynamic factor known as vital force or bathmism or human will or animal instinct or helio-

tropism or metabolism, etc., the social scientist deals with interaction between physical and cultural or social environments, as well as with a play between physics and psychics (supposing we grant this line of division at all).

For this reason a marked difference between law and correlation exists. The first has a small and definite number of units conceived as irreducibles. Its qualitative and quantitative relations may be established by approved methods, and the conditioning elements usually be subjected to a like exact measurement. The second group however, viz., our correlations in bio-metrics or physiology or social science, rest on highly complex units, on what the physical scientist would call compounds. Variability follows from the complexity of the units, while the interrelation between physical and non-physical data accentuates the difficulty of finding a true generalization. Again we may start where we please, and wherever a marked quantitative negative or positive correspondence appears, there the grounds for induction are provided. Thus I may correlate the weather with passenger-receipts or with employment facts, or with suicides or acre-yields or with the efficiency of laborers in a mill. All this is logically tenable and may lead to useful information. If natural science has a free field for examination, how much more so the biologist or social philosopher! But both law and correlation are derived from units *taken for the time* as irreducibles and realities of the outside world. We have a correlation in both cases, though the unfailing regularity of a law, being demonstrable by standard devices, exceeds greatly the proximate regularity of correlations. There is qualitative as well as quantitative correlation, absolute facts being connected in time and space. There are

relative rates of change, and constants of such interdependent magnitudes. And once more, while the conditioning factors are rarely measurable in correlations, differing in this respect from the accompaniments of a law of nature, yet the mere circumstance of a conditional environment common to both is equally noteworthy. For it means that correlation no less than law is an abstract. It means that the subjective element remains in both, though in different strengths. It means that there is no more reason for denying the uniformity of conditioning phenomena in social science than we are likely to deny it to the perceptual events cradling, as it were, the laws of nature.

If a further distinction therefore is to be made between law and correlation, it cannot hinge on their inward nature, but only on the units of events and time within which each set of events reaches a regularity of recurrence. The choice of these temporal and spatial *units* is verily significant; for absolute time and space may be considered irrelevant for our problem. What counts most is the circumstance that our laws and correlations are hedged in by finite amounts of these infinities. We deliberately relate our correlates of physical or other complexes to such doses of time and space. One becomes a function of the other. We take the reality of time and space for granted and then snip off fractions to serve as a vehicle for our quantitative expressions. Thus, although the outer limits of time and space for the possible laws of an organism would be its life and the earth, the experimental time and space units for natural scientists are relatively small. But in the social sciences both time and space units may be very large, and usually are rather poorly defined, so

that the difference between sequences and co-existences may scarcely be ascertainable.

What is Causation?—Hence a problem also arises as to the place of temporal or spatial units in a causal analysis of both physical laws and statistical correlations, a matter that will lead us to inquire into the true relation of causality to law, respectively correlation. We may ask: Are law and causation different entities, or not? Is cause-effect a part of law, but not of correlation? Or should we decide differently?

It will do no harm to begin with a few representative quotations from works which are notable for their masterly treatment of this vexing question. We shall then appreciate at once the identity of law and causation as seen from one particular angle. Thus we read with interest statements like these: "When we say that every effect has a cause, we mean that every event is connected with something in a way that might make somebody call that the cause of it." [10] "There is no particular difference between knowledge of causes and our general knowledge of the combinations, or succession of combinations, in which the phenomena of nature are presented to us, or found to occur in experimental inquiry." [11] "A cause is not to be distinguished from the group of positive or negative conditions which, with more or less probability, precede an event." [12] "Things are not either independent

[10] Clifford, W. K., "On Aims and Instruments of Scientific Thought," 1872; Becher, E., "Naturphilosophie," 1914, p. 148.

[11] Jevons, W. S., "Principles of Science," Book II, § 1.

[12] *Ibidem.* See also Gibson, W. R. B., "The Problem of Logic," 1908, p. 372. For an early statement see Reid, Th., "Inquiry into the Human Mind on Principles of Common Sense," 1764 (Sneath, E. H., editor, publ. by Holt, H., 1892), p. 332. A recent suggestive criticism of the popular view of causation is given by Campbell, N. R., "Physics, The Elements," 1920, pp. 57-70.

or causative. All classes of phenomena are linked together, and the problem in each case is how close is the degree of association." [13] "The origin of the concept of causation is now manifest. It is that of the part, explaining the whole—or, avoiding this untechnical use of 'part' and 'whole,'—it is that of *some* explaining *all*." [14] "It is involved in the causal relation that if two things are really cause and effect, the one never exists without the other." [15]

In other words, to ask what is the cause of this, what is the reason for it? Why did it happen? is to ask simply: What else goes with it or follows or precedes? What is it that regularly or ordinarily, so far as our experience tells, forms part of the chain of events of which the event known as effect is also a part? This is the real meaning of our inquiry about the why and wherefore. We want things or happenings connected in a series which regularly recurs either exactly as witnessed by our senses, or in the form which science by degrees sifts out as the quintessence of a law of nature. But let us not forget that the events must have a name or must be *specific*, so we may recognize them, setting them aside from other events. If we were to assert that causation means merely a succession of facts, without attempting to itemize their characteristics, we should not have ventured very much. It is not any x followed by any y that engages our at-

[13] Pearson, K., "Grammar of Science," edit. of 1911, vol. I, p. 166, and pp. 157, 173.

[14] Whitehead, A. N., "Inquiry Concerning Principles of Natural Knowledge," p. 187.

[15] Joseph, "Logic," p. 429. His classification of causes is given on pages 459-62. See also Schiller's excellent tabulation, "Formal Logic," ch. 20, § 7. For a dynamic view of causes as variables see Russell, B. "The Analysis of Mind," 1921, pp. 93-98. On plurality see Joseph, "Logic," ch. 22, and Venn, J., "Logic," p. 62.

tention, but some distinguishable known *a* and *b*, or specific sets of events such as abc as antecedents, and *def* as consequents. Everything happens in bundles or groups, not as a line of individual events separated by long intervals of time. Specific causation therefore is our topic, not an attenuated theorem of Uniformity of Nature such as logicians must postulate when they talk of induction and the roots of human knowledge.

Now, of this sort of causality a huge literature has treated for many centuries, and yet some problems have remained unsolved or in any case seem still susceptible of more than one solution.

To begin with there is, e. g., the old query whether coexistences can be causal or not. The majority of logicians has counseled the rejection of a causal status for coexistences. John Stuart Mill's attitude has been fairly typical in this matter as in some others. He assures us: "The law of causation—is but the familiar truth that invariability of succession is found by observation to obtain between every fact in nature and some other fact which has preceded it . . ." [16] "There are certainly cases in which the effect follows without any interval perceptible to our faculties," but "whether the cause and its effect be necessarily successive or not, causation is still the law of the succession of phenomena." [17] Such was the great Englishman's view, and one suspects that his training in an older school of metaphysicians was partly responsible for his verdict. For if we may believe the writers quoted a while ago, or if we proceed to examine at close range the structure, the anatomy so to say, of law and correlation,

[16] Book III, ch. 5, § 2.
[17] *Ibidem*, § 6.

we shall not be inclined to predicate of sequences more than of coexistences. But of this a little later.

Another point about causality, as students are well aware, is the rather arbitrary fashion in which we single out certain events as causes (respectively effects), without realizing it at the time. The common-sense man errs here as he does on other occasions. He sees perchance a leaf dropping from a tree, and on being questioned as to the cause, says: The wind, of course. It is clear to him that no other explanation is as plausible. He takes many things for granted, such as the rotting of the fiber that for long months fastened the leaf securely to the twig, or the law of gravity, or the angle at which the leaf was struck by the breeze, or the condition of the tissue in the leaf which made it wilt and curl. These sorts of facts do not interest him. He overlooks them in making his diagnosis and does well in doing so. But note that by this route an end result becomes an effect to a cause which itself is only one of a variety of elements to be considered by us, were we to be scientific, and not practical. Motion and action, for that matter, usually arouse our attention and prompt us to pronounce them causes. Or we assume a certain set of facts because we become used to them, *picture them as constants*, and then pounce on a detail as the variable or cause. Just as primitive folk associate causes with gods and designing human beings, so movement appears as an *agent* setting off effects at a given time. This is one mistake that trained minds will not be long in disclosing to the unsophisticated. But it is not the only one, nor perhaps the worst.

For equally indefensible is the belief that there may be a plurality of causes, and yet not one of effects—a notion

that runs like a continuous thread through a considerable literature in logic and philosophy. But why should we insist upon such a distinction, once we have studied laws as bundles of units of a more or less elemental character? Plainly, if it is true that many causes of death exist, or that in this sense events do not occur in a reversible order, so also should we understand that any one cause has many effects, *if the effects are analyzed as minutely as* our alleged causes. That is, if we reduce effects to as small units as our causes, then we have either a perfect reciprocity of causation, or we have none at all, because as *perceptual* events causes and effects are never altogether the same, however similar their recurrences may seem to be. Thus to argue for many causes of one consequent called death is to forget that death is an event-complex built out of scores and hundreds of smaller units of events, each one of which according to the mechanics of natural science must have just one correspondent in the group of antecedents styled the causes. Not to argue for the variety of causes in such an article as poison, let us be mindful of the almost infinite number of consequents connected with the swallowing of it. Why make death one event, and then link it with a host of different events leading up to it, such as disease or accident or a paralytic stroke or the taking of poison or a deliberate shot from an assassin's gun? It is manifest that more units of things and more magnitudes of our events in the complex are involved than a first thought would suggest. We have actually a congeries of laws, all operating in the act called the dying of a person, but appearing to us as conditions which are mainly slighted or not noticed at all. On the one hand, then, conditioning factors may

be pointed out as causes of a change in a select chain of events, while on the other hand they may be ignored entirely, our interest centering in but a single culmination observed as surcease of life, that is of motion or thinking or feeling or breathing, etc.

But this flaw in the argument for a plurality of causes in contradistinction from a singleness of effect should aid us in gauging again the subjective aspects of causation, as well as the possibility of calling the correlates in a law of nature either cause *or* effect. The hypothetical, artificial nature of a causal imputation is indeed undeniable. "We must always assume a considerable amount of preliminary information as to the nature and limits of the field over which the cause is to be sought. That is, the claimants to that post must be supposed to be finite in number, and to have all their names previously submitted to us, so that we have merely the task of choosing amongst their respective qualifications. In fact, we must assume more than this; for unless the possible causes are extremely few in number—so that all their combinations can be taken into account, we must take it for granted that we have some indications given to us as to which are the serious claimants whose qualifications only have to be carefully tested." [18] Hence "the greater the scope of existential knowledge, the greater is the likelihood of our being able to pronounce events causally dependent or independent." [19] In most cases "those elements which we are apt to regard as separate antecedents, isolating them and representing them by means of letters, are largely the results of our own more or less artificial construction by

[18] Venn, J., "Principles of Empirical or Inductive Logic," p. 431. See also Schiller, "Formal Logic," pp. 281, and 293-96.
[19] Keynes, J. M., "Treatise on Probability," p. 277.

abstraction. There is nothing strictly corresponding to them in nature." [20] Or in the words of still another author: "What the 'cause' of an event is (or is called) depends on the speaker's interest and the purpose of his inquiry." [21] "The choice of cause depends upon the social interests" of people, and we "make a choice determined by the frequency or the interest of certain conditions in comparison with others." [22]

So far so good, then. But the subjectivity of causal relations may be shown also by the delimitation of time units within which causality is held to reign. We admit for instance that sometimes the interval is so small as to be imperceptible. Whether friction is a consequent of motion, or is a concomitant, strictly speaking, who will decide? Whether the explosion does really take place after the ignition of the powder, or is part of this act, who cares to measure the lapse of time in between? The question will possibly be deemed an unanswerable one.

But more. Our time units in sequences are artificial in two principal respects, viz., first as an absolute span of time within which the law takes place, and secondly as the intervals between what is called cause and effect. We decide arbitrarily upon the temporal framework within which, say, a thunderstorm occurs, pointing to a "beginning" and an "end"; and we also set limits consciously or unconsciously to the quantity of time that may separate a lightning bolt from a thunder-clap. Always the present moment becomes a *terminus ab quo*. Regularly we refuse to trace our series of events beyond a certain point of the future. Why a thunderstorm begins here and ends

[20] Venn, "Logic," p. 77.
[21] Schiller, "Formal Logic," p. 277.
[22] Enriques, F., "Problems of Science,' pp. 142.

there it is impossible for us to say, except that motion and commotion, sense data heaped up in a short stretch of time, impress us as a logical unfoldment of the phenomenon in question. That cycles of such event-complexes might be interlaced, that the intervals between two successive storms might constitute a unit for causally relating its components, does not occur to us. Yet we may profit by asking something like this: Is the climate a cause or an effect of the contour of the land where it prevails? Which comes first and which last, if you please? Pondering a little on this we shall have to admit that either or both will do for a reply. Looked at from one viewpoint the weather alters the landscape, acting by erosion and corrosion, so that mountains are reduced to valleys, and the shore may be built up from alluvial deposits. But it is equally fair to point out that topography makes climate, since altitude and floral conditions and contour and soil qualities regulate radiation, condensation, sky-conditions, temperature, etc. Dependent upon how we select our time units, one or the other set of facts appears as cause. In the realm of *physical* phenomena a reversal may thus be defended. All is either cause or effect, or neither.

Causation and Correlation.—That of course brings up a somewhat different question to which we may now turn in order to complete our study of law and causation. Namely, we are bound to ask: Does causation also rule in the world of non-physical events, for happenings such as the biologist, psychologist, and social scientist makes his specialty? Is a reversal possible here too? And if we treat causation simply as another way of expressing law, shall we draw a hard and fast line between natural

and other sciences? What seems a right answer to these queries?

Now, in the first place it should be definitely understood, since agreement on this point has been reached long ago, that no conflict can be trumped up between freedom of the will and causal determinism. For certain purposes, and not least of all the theological, the human will may be treated as a distinct entity, as a vital force, that is part of a larger personality and rises far above the level of causal happenings. Undoubtedly there is a sense in which man is a willing being, an independent agent who directs his conduct as he sees fit and shapes the routine of experience according to his intentions. It is altogether fair that we take this view of the question at times, if only to provide a ground for an abstract ethics that serves to acquaint people with possibilities otherwise unsuspected. Illusions surely are often as useful, as necessary to achievement as the plainest of truths! However, for the scientist the will cannot figure as a power exempt from the laws governing events in general. For him it is clear that *will is but a particular way of rating the variability* that marks organic matter and human beings in particular. From a scientific standpoint will is one aspect of a situation whose components are as capable of correlation, as intimately bound up with physical forces, as the lowest forms of plant and animal life for which consciousness may be said not to exist at all. To me as doer my will is real. To the spectator also it represents a propelling force whose rôle is unique in the cosmos. But seen from another angle will is only a term for a wide range of variations, for adaptability to constants in the physical world. To will, thus, is to act rather than to

plan, and our degree of freedom must be measured by the trend and scope of changes which constitute human history itself.

In the second place the causality of correlations, as compared with exact laws of nature, is obscured by the indefiniteness of the temporal and spatial units usually involved. In our laboratory work we deal with fairly definite spans of time, both in conducting the experiment as a whole, and in allotting limits to the intervals connecting a given sequence. But the more we pass from physical facts to the social or historical, the vaguer the boundary-lines assigned to our correlations. Correspondingly our idea of cause or effect changes as our time units change. A war for instance: What are its causes in the opinion of discerning men? Leaving aside the fact that the very nature of our material permits us to choose a large variety of data for a grouping into coexistences or sequences, we are further obliged to grant that much depends upon whether we take a short or long-time viewpoint. To contemporaries the causes are probably in plain sight. They *know* that certain personalities or institutions or creeds or foreign policies or incidents of a tangible sort are responsible for the catastrophe. They have no hesitation to point a finger at the *casus belli* and to date the commencement of the war from those near-at-hand events. Not that the opposing parties will agree even then—for the force of bias in such matters is axiomatic!—but at any rate each side will have its explanation, selecting details that the popular imagination no less than that of the trained observer may seize upon. But let a few decades or a hundred years go by, and the values are transformed so as

to be unrecognizable. What once seemed a prime factor now has no standing whatever; what formerly was not even noticed, now looms up as an event of cardinal importance. Thus the same set of antecedents get a different causal rating, and thus different events are brought into correlation from one era to the next, until perhaps after centuries a general consensus of opinion is reached. Causation thus proves to be in fact what the philosopher has known it to be in theory, namely, a way of ordering events so that they invariably or with a tolerable degree of regularity recur together. Even for the historian it is possible to establish regularities if he reduces event-complexes to the primary relations of their physical or psychic components; and for the social scientist the search for typical transactions is of course a first duty. Or if we deny the historian the right to hunt for types of events, we must yet allow that all event-complexes may be correlated by sociologists or economists for the purpose of finding more or less perfect recurrences. It will depend upon the degree of quantitative or qualitative correlation whether causality is attributed to it or not; but that is not to deny the possibility of such a causal nexus.

The *make-up* of the events themselves is however, in the third place, equally significant for our traditional distinction between causal and non-causal events. For owing to the complexity of the units correlated by social scientists or biologists or psychologists the basis is lacking for that one-to-one correspondence that apparently pervades the whole physical world. As we pass from non-organic to organic phenomena, as we pass from floral or faunal facts to the social in their multitudinous aspects,

we are compelled to reckon with a variability, with combinations of events, each counting as a single unit for our requirements, that make their reduction to indivisibles or "atoms" impossible. The generic difference between animate and inanimate elements is quite incontestable. The compound nature of the events which we press into a formula for generalization suggests itself on first thought. To correlate sizes of the leaves on a tree, or the weather with the migration of birds or with human wanderings, or prices with types of social structure, or the distribution of plants with insect life—to do this is to work with units which manifestly are resolvable into much smaller and simpler ones. The social investigator would concede this no less than a physiologist or chemist. But this being so it follows that our sense of the non-causal character of such correlations is merely due to ignorance. We admit that the mechanistic view is not directly applicable. We see difficulties aside from the instability which marks organic behavior or the expressions of a social group. We are confronted not only with an indeterminate number of variables, but likewise with many unknown qualities and quantities, some of them again compounds, others imaginable as raw-materials for natural science. We say we cannot locate the "causes." We perhaps argue that no causal connection exists, meaning that the compounds surveyed do not evince the kinds of reciprocity, the measurable quantitative interdependence, inherent in the units of physics. Of course, so we should put it. But since cause and effect in any case are only names for sets of events within a stated law of nature, since with one exception to be mentioned later we may arrange our time-units so as to make cause and effect

interchangeable as units within that law of nature, there is no reason for pitting physical against social events, as if they were irreconcilables. Nor need we waive the right of further reducing our units in the groups, so that a causal attribute may somehow be revealed in our correlations.

Or to state this thought more concisely: We may consider an event-complex explained in so far as we have referred it to known "causal" values obtaining for the smaller units out of which the complex is composed. Thus if we wonder whether a high positive coefficient of correlation between the educational status and the criminal record of a country can be causal, we need only to define our terms education and crime, and then search into human traits and actions that are more or less regularly connected. Or we may trace these ideas, motives, and habits of education and crime further back to biochemical groups known in physiology; or again we may go from sociological data to the biological, and thence to perhaps the physico-chemical. In some such way we actually do reason, and this regress back to the simpler units handled by natural scientists is all that *can* be done to establish a causal-nexus in our statistical social correlations. To say that they are not causal is to contrast their compound units with the regularity of the simpler elsewhere, which we have arbitrarily beforehand designated as causes or effects.

Whether anything is gained by this regression is another matter. It may appear that in so converting correlations into physical items we have really changed the nature of our inquiry and lost what we originally set out to do. But evidently some such system of indirect link-

age may be used to interlace any class of events with a second or third or tenth, until physical "causality" emerges. Day and night thus are as causal as any constituents of a law of nature. To pronounce this succession non-causal, as logicians have done again and again, is to misunderstand the hypothetical, pragmatic character of causation in general. A great many correlations are "empirical" [23] laws in that their final units have not been disclosed. We see regularities and cannot trace them back to known regularities of other groups of events; or we are struck with the imperfect degree of stability, with the variations in detail that bid us to proceed carefully before formulating an exact law of nature. Such distinctions of relative constancies, of groups of recurrences according to kinds and variability of their units are quite necessary, but they should not, they cannot, prejudge the case for causation. Causality applies either to correlations as well as to laws of the physical sort, or else we admit it to be a convenient term merely which helps us to differentiate the elements in a law or correlation, the temporal and spatial units for this purpose being varied as we see fit.

But will this two-sided reading of our successions of physical events oblige us to extend it also to a correlation of physical with non-physical events? And furthermore, what becomes of chance correlations if we repudiate so uncompromisingly the old notion of cause and effect? A few words on these topics will not be inappropriate, although they can add nothing to the essentials already brought out.

[23] For definitions of empirical law see Mill, "Logic," Book III, ch. 16; Bain, A., "Logic," 1874, p. 333; Hibben, J. G., "Logic," 1904, p. 351.

As to the reversibility of events, there should be no doubt about the difference between connecting physical events alone, and linking them with what are called psychic or social events. And of course, the correlation of psychical or social events with each other is also a distinct principle. Thus, if we ask ourselves whether density of population and climatic conditions may in any way be causally related (giving the word causal its traditional meaning), the answer will be: Certainly. It seems reasonable to attribute facts of population to facts of climate, and so men have done often enough. But may we then go from population as a cause to climate as an effect, in the sense that we have, awhile ago, made *either* topography *or* the weather both cause and effect? Now, in so viewing the problem we are reminded that a notable difference exists. We shall argue: The climate surely may account for demographic data, but these latter have no bearing upon the meteorological data. This seems assured. But what is the reason?

We must answer that life phenomena do involve a power for variation, a range of adaptation as the biologist or psychologist would say, which make a dual tracing of antecedent-consequent impossible. There is a real and important gap between organic and inorganic facts, or at any rate between the latter and the higher forms of life. The units of our correlation of vital phenomena—biological or sociological—are effects only, and not causes in our illustration because of their very complexity and evolution out of simpler elements prevailing in the realm of physics and chemistry. There is, in other words, a principle of evolution from electrons to ideas which must be understood before the line between dual and single causa-

tion can be resolutely drawn. We must realize the derivation of the complex units of statistics, or say of the life-sciences, from the irreducibles of natural science, before seeing the consistency of our distinction. Any one set of physical events may be either cause or effect, but as between such and psychological or socio-economic facts, we shall have to grant that psychics has no influence upon physics. Or, rather, since this is not strictly true, let us say that *within* any one organism psychics and physics are linked by a dual causality so that each group of facts may appear as either cause or effect. But otherwise physics acts only on psychics, and not vice versa; or physics acts only on physics, or psychics only on psychics, possibly with the accompaniment of physical changes in the organisms concerned. But that is neither here nor there. The main principle to seize upon is the limit within which causality is reversible. And aside from that, we might note also that a plurality of causes will prevent us from attributing demographic facts to climate alone, while on the other hand we should be willing to acknowledge a causal connection between variations in climate and population features taken strictly in a physical sense. It is merely the immeasurable superiority of cosmic forces over the physical quantities represented by, and interacting within, individuals and their aggregates that bids us to read causation here in one direction. We ascribe functions solely to climate which on a minor scale exist also for the physics of social life, and in precisely the same causal sense.

All this is quite understandable and in accord with our next remark that chance correlations implied or explicit are impossible from a scientific standpoint. The word "chance" clearly is a misnomer if a Uniformity of Nature

prevails, and is wrong even when referred to specific causal connections such as are embodied in laws of nature or in ordinary statistical correlations. There must be causation everywhere, or else it is nowhere. Since cause and effect mean merely regular connections of designated classes of events, all regularities meeting certain standards must be causal. But, of course, we may measure degrees of regularity of the recurrence of groups of events, and deny "causality" to some of them in so far as the degree is below a fixed—or more or less fixed—minimum. That is entirely possible and indeed represents the actual state of things. The proportionate quantitative changes of two or more homogeneities that do not repeat themselves with sufficient uniformity we decline to call real correlations or causal connections. Statistical induction, in such cases, is out of place. Our coefficients are said to be unsatisfactory because too variable or too low. But this is not to deny causation in general, nor to accept popular opinion on "accidental" happenings.

Conclusions.—Our conclusion on the whole subject thus will be as follows. First, law and correlation have much in common and may be treated under one heading, even though differences in degree will become conspicuous. Second, causation is for both or for neither according to viewpoint and definition of the term. Third, a subjective element permeates all generalizations of science, but this forms no bar to the objective reality of a substratum of facts out of which the human mind builds its laws of nature. And fourth, the differences between law and correlation call for differences in method which it will now be our task to examine before stating the methodology of economics in particular.

CHAPTER EIGHT

THE METHODS OF SCIENCE

Preliminary Observations.—As remarked before, in discussing the methods of science it is well to distinguish between inference on the one hand, and measurements on the other. Inference is not something peculiar to scientists. On the contrary, it is the common property and practice of all people, being a part of human nature and an element in all social life. Indeed, if we wish to give a relative rating to these two phases of scientific progress there is no doubt that for the earlier stages of history mere inference must have been much more important than those exact measurements which, in historical times, have helped to make us masters of our natural environment. At the beginning everything depended on a rough appreciation of differences and resemblances, that is, on man's reasoning by analogy and on induction by enumeration. Method in the larger sense is this use of inference for purposes of adaptation to practical requirements.

In the narrower sense, however, we must define method as a more or less clearly marked procedure for discovering fixed relations between things, this procedure necessitating often the use of instruments for measuring facts and their changes. Any methodology of science therefore must take notice of three distinct sets of facts, viz., of infer-

ence in several aspects, of certain types of measurement by which different kinds of subjects are made a study for science, and of the results attained by science, particularly as laws of nature or as causal relations among specified events. Methods for different sciences may vary in so far as the points of emphasis differ, and differences in subject-matter and in aim bid scientists to rely upon one scheme of measurement or another; but inference is everywhere the same. To use in- and de-duction scientifically is simply to turn both to better account than a common-sense viewpoint could promise.

The methods of science resolve themselves on closer inspection into three standard ones, unless indeed a mere classification of properties, because carefully undertaken and sufficiently complete to cover all distinguishable features, is itself called a method. If we assent to this plan then many sciences will of course work independently of measurement in the precise sense of the word. But otherwise there is good ground for observing a threefold division, the first being experimentation, the second statistics, and the third reflection. In other words, barring the purely classificatory disciplines such as botany was until the last century, sciences reach their conclusions either through experiment, or through a counting and assembling of large numbers of like facts, or through an introspective analysis which, though resembling in part the first two methods, has peculiarities of its own.[1]

Aids to Science.—Experimentation has been called the method most characteristic of physics and chemistry, and this is undoubtedly true. It must however be noted at the

[1] For a conventional analysis of scientific method see, e.g., Lodge, R. C., "Modern Logic," 1920, ch. 18, or Sellars, R. W., "Essentials of Logic," 1917, chs. 16-18.

outset that at least part of its work is being done just as well by the statistician or the reflective thinker, although outwardly this does not so appear. Namely, if it is true of natural sciences that they depend largely on observation and trial and error, this is no less true of other fields of inquiry. Indeed, it follows from the nature of human thought, and from the principal facts of reasoning, that observation and trial and error antedate the methods of measurement now most generally associated with exact science.

What observation involves is well known. We have to deal here with men who, whether scientists or not, use their senses to perceive events occurring outside, to compare them as to resemblances and differences, to classify attributes or events on this principle of likeness and unlikeness, and to remember as much of the situation as seems necessary for certain practical or purely scientific ends. In the last analysis observation is a crude kind of abstraction. It is attention directed to a select group of data, of happenings or appearances, which we try consciously or unconsciously to bring into an interdependence different from that of time or space alone. We start with comparison and distinctions guided by our senses, but quite usually wind up with a judgment of causal relations or even with generalizations that do not in tenor or purpose differ materially from those of science. But one fact deserves to be noted in setting observation apart from scientific work, namely that the former relies exclusively upon sensation and perception, so that the limits of our knowledge derived purely from observation are proverbially unsatisfactory. We may of course remember much of what we have observed, and so extend greatly

the range of our analysis by combining images centrally aroused with the picture before our very eyes; but this is still a process differing in essence, and not simply in degree, from what the scientist aims at.

Trial and error is, in this respect, no better than observation, for it too relies chiefly upon sense experiences at first hand. Whether we turn to a practical problem that calls for immediate solution in the course of our everyday needs and interests, or whether our experiment deals with topics remote from the possibilities of practical application, the primacy of perception and the absence of generalizing concepts is apparent in both situations. The common-sense flavor of trial and error is a characteristic that appeals to all and correspondingly links it up closely with mere observation.

It differs from observation however in that we undertake purposely certain variations so as to find a solution of our difficulty, that is, to trace what in logic and popular parlance both is called cause and effect. We look for correlations of a particular sort. We wish to obtain light on facts that constitute a departure from the ordinary. Or in other words, in trial and error we are usually confronted with a new turn of events to which we desire to adjust ourselves perhaps on the instant. The facts to be appraised and correlated are practical in most cases, although not necessarily so. Furthermore, in meeting this contingency, we have neither time nor inclination to expand our problem, that is, to generalize consciously so as to unravel a skein of relations indicative of laws of nature. To find laws is not primarily the function of the matter-of-fact man who proceeds by trial and error. The conditions are given in the outside world and are not, in the

majority of cases, reducible to anything like an orderly array of selected facts. No desire arises to measure carefully the quantitative changes that might by a scientist be discovered. The aim is to overcome obstacles, to find out why the way in which we have worked before does not give the wished-for result this time. Trial and error thus is bent on measuring the effect of interferences, of subtracting from the whole situation that element which shall be amenable to our usual methods. Novelty has to be studied and resolved into familiar connections. We proceed haphazardly, without studied hypothecation or the use of instruments other than such tools as a workingman would use to turn out his product. Instead of following habits we rely upon our resourcefulness. We are led involuntarily to experimentation, and like animals, whose ingenuity is limited and not easily wrought into a systematic adaptive scheme, we toy with the facts at random, guided only in a meager degree by previous experience or by suggestions that approach science in their definiteness.

Now, while these two preliminary stages of method are common to all three standard methods—or rather, though each method involves observation and trial and error in some aspect—experimentation proper is greatly superior to them. For experimentation is that kind of research which achieves most by methodical measurements and additions or subtractions to and from a complex of events, things and attributes being arranged expressly for that purpose. Experimentation, thus, is a variety of research which does what trial and error scarcely ever presumes to attempt: It selects its data. It arranges means by which to control the changes in these data. It resorts to

the use of instruments by which, directly or indirectly, measurements of maximum nicety are made possible. It varies the data under investigation at will, as much and as often, as regularly or as irregularly, as seems expedient. It starts with assumptions usually, or makes use of them some time during the act of research. It endeavors to ascertain permanent relations, thus projecting itself beyond the realm of sense into that of concepts and the imagination. It creates difficulties rather than dodging them. It puts itself in the service of others, of mankind in general. And finally it tries to unify an immense diversity of laws or theories into a system. Experimentation therefore is the most prominent of scientific methods, and the gateway through which men have entered into a kingdom of thought and economic treasures that grows with each successive generation.

The use of standards is one of the characteristics of science, and must be regarded as of primary significance particularly for natural sciences. Physics and chemistry, astronomy and biology, are fields in which the use of standards is most obvious and decisive, but other sciences also have them, although a notable difference appears at once. For while natural sciences detach their standards from their subject-matter, giving them an objectivity that is genuine and beyond cavil except perhaps from a philosophical viewpoint, social sciences must in part reckon with norms which themselves constitute part of the data to be measured, so that complications arise that a physicist knows nothing of.

However, even for natural science two facts stand out in plain view; namely, first, that relativity is a basic postulate as well as a fact empirically verifiable, and sec-

ondly that all standards anchor, at bottom, in a few primary concepts which in turn are no more than definitions. Thus, on the one side, we might learn from the now current speculations about Specific Relativity, quoting the words of Einstein, that "there is no such thing as an independently existing trajectory (path-curve), but only a trajectory relative to a particular body of reference." [2] In this sense all measurements presuppose a standard by which others find their value; and this meaning of the relative worth of scientific standards would be as suggestive for social sciences as for the physical. But on the other side we must remember, more especially, that the great majority of standards are compounded of simpler ones which themselves go back to definitions arbitrarily given, no matter how "exact" our results. Thus, to illustrate by one example: If we open a text on physics at random, happening to strike the term "kilowatt," and ask ourselves what this means, we shall get an answer somewhat as follows. To wit, the kilowatt is a thousand watts, and the watt "the power possessed by an electric current of one ampere under a pressure of one volt; this latter being the electromotive force which will cause a current of one ampere to flow through a resistance of one ohm." Now, what is the ohm? We are told: "The resistance offered to a current of electricity by a column of mercury 106.3 cm. long, having a mass of 14.4521 gram at a temperature of melting ice"; and the ampere is the current which, when passed through a proper solution of silver

[2] Einstein, A., "Theory of Relativity" (translated by Lawson, R. W.), 1920, p. 10. See also Ames, J. S., "Presidential Address before Physical Society, December 30, 1919; and Eddington, A. S., "Space, Time, and Gravitation," 1920, p. 8: "Natural Geometry is the theory of the behavior of material scales."

nitrate, will deposit upon the cathod .001118 gram of silver in one second. The gram in turn is the one-thousandth part of a kilo, which itself is supposed to have the same mass as that of one thousand cubic centimeter of pure water at 4° Celsius, zero being the freezing point of pure water at sea-level, while the boiling point is one hundred degrees above that.

This, of course, leaves us still the centimeter and the second, to say nothing of other terms which might be said to call for definition, if our idea of a kilowatt is to be rounded out in all directions. However, waiving these further explanations, we proceed to the two remaining definitions, and find first, that a centimeter refers to a linear standard which is the length of a bar of platinum in the city of Paris, in France, this bar containing 10 per cent iridium at O° Celsius; while the second is a fraction of the solar day, whose prototype is the sidereal, that is the time interval between two successive returns of a fixed point on the earth to the meridian. Thus a reliable unit of time measurement is established for which astronomers have vouched, and so our kilowatt becomes intelligible as a derivative of simpler units which terminate somewhere in definitions, that is agreements.

Standards are important for science, since it frequently quantifies qualities, going in this respect contrary to common sense. Quality and quantity are of course aspects of one and the same thing, and therefore should not be separated as if it were impossible to treat a fact from both viewpoints. In reality there is no class of things whose quantities or degrees of quality could not be compared in a continuum of measurement. Quality and quantity are categories like time and space that represent modes of

thinking, if nothing else, and permit us to bring order into chaos.[3] But it is significant that the impressions of our senses may yield qualities when measurements of science demand quantity. The treatment of color and sound as wave length and rates of vibration are old illustrations à propos of this subject. The curves which to the eye seem so entirely different from straight lines are resolved into points by mathematicians who see continuity even while postulating discreteness. But let us not forget that it is as easy to make classes out of different sizes of an object as it is necessary for a physicist to reduce quality to point-events. For one purpose there are as many qualities as we have sensory nerve endings; for another the number of qualities depends on our sensitivity to degrees of intensity in stimuli; and for yet another purpose we may feel obliged to picture the whole world as an infinite —or finite!—number of atoms whose perceivable interactions alone mean quality and individuality. Organic beings, for instance, may always have to be regarded as indissoluble units, since laws of life function in each separately and create differences which for practical purposes are inextingushable. We know, e.g., that ten mediocre men do not equal one man of genius, and that the interests or actions of one do not depend altogether upon those of a second or third. Yet these facts do not prevent natural scientists from quantifying certain sense data, nor are they inconsistent in harping on quality or attributes at other times when their search for laws urges them to.

Experimentation as the First Method.—The method of science, and particularly of the natural sciences, is there-

[3] For logical aspects see Bosanquet, B., "Logic," vol. I, p. 127.

fore both a qualitative and a quantitative analysis.[4] Events are described as happening together in time or space, and by event we may mean either an object discernible by the senses, or something *imperceivable*. The chemist correlates substances or qualities as well as quantities of each element. In his case the two phases of measurement are plainly visible. But they exist just as well for physicists or other investigators, although the quantitative view may be most noticeable. In all cases of experimentation students aim at an isolation of things and their relative amounts, changes under more or less complete control of the factors in question being narrowly watched and recorded. *Science cannot attain to certainty unless it succeeds in finding irreducible units, that is, things or magnitudes which invariably go together and so provide the basis for the formulation of a law.* Irreducible units are as essential to exact law as atoms to chemistry. The final outcome of all inquiries must always be the detection of that smallest number of elements or events which regularly occur in succession or simultaneously. It is by isolating the facts which give this simplest formula for event-complexes that the natural scientist fulfills his chief function.

The method of variation [5] is hence a standard procedure of physical scientists. Quantities are correlated as absolutes or as variables; and the variations are found

[4] On significance of twofold measurement see: Spencer, H., "Genesis of Science," in "Illustrations of Universal Progress" (Appleton & Co.), 1890; Pearson, K., "Grammar of Science," 1911, p. 173; Westaway, F. W., "Scientific Method," 1912, p. 214.

[5] Treated usually from standpoint of causality, and made familiar by Mill, J. S. For excellent tabulations see Schiller, F. C. S., "Formal Logic," 1912, ch. 19, and p. 265; and Joseph, H. W. B., "Introduction to Logic," edit. 1916, pp. 439-40.

either to express constant ratios, or to approximate them. Whenever fixity of proportion, or constancy of correlates is lacking, we must infer that the latter are compounds and need further reduction to simpler units. In making note of these extraneous elements which, from the standpoint of any one central correlation, appear as "conditioning" factors, we may be able to find other laws of nature. Variation amidst changing known or unknown events becomes necessary whenever a "conjunction of elements or features in the real [world], whose connection is not intelligible from a consideration of themselves, is made clear through connections shown between them and others." [6] A plus and minus is introduced, whose bearing upon the problem under review is itself treated quantitatively or qualitatively, according to needs. What logicians for generations laid down as the Canons of Induction, is this addition and subtraction of events, the differences or agreements being checked up so as to help us establish the correlation of "essentials," i. e., a law whose regularity should equal our notion of immutable necessary connections. Logicians, to be sure, have worked at this problem with a desire to explain causality, or to show how things are proven to be *the* cause or the effect. But it follows from what has previously been said that a more modern and just estimate will ignore the old belief in specific causation, confining itself instead to the fact that invariability of sequences or coexistences does appear, or where not in evidence, would prevail except for interferences which themselves obey laws as truly as our particular correlates.

In general the experimental method revolves about this

[6] Joseph, "Logic," p. 502.

weighing of alternatives, of things common to several consequents or antecedents, and of unique properties which in any one case indicate what is regular and what is not. "Inductive conclusions," as one writer has aptly stated, "are established disjunctively by the disproof of alternatives." [7] Science uses the disjunctive syllogism more than any other. It uses it in measuring the integral parts of a complex to be analyzed as a law or as a set of laws. It uses it to measure the conditions as well as the problem itself. It compares, in a precise quantitative manner, all changes subject to control, and generalizes in due time on their significance. Experiments are repeated a few times or a great number of times, dependent upon the number of events under investigation, upon the degree of regularity tentatively ascertained, upon the novelty of inferences suggested, and upon the importance of results theoretically or practically. In most cases it is inference from enumeration, as well as a comparison of partial or total resemblances, that leads to substitutions or reasoning by analogy. Hypothecation is indispensable in most experiments. An hypothesis entitles the student to make deductions, to continue his measurements and see whether his deductions agree with the facts so discovered, and to pronounce his inference "verified" if the agreement is complete. Or rather, since a consequent may have several apparently identical antecedents, an important duty of the experimentalist is the right choice of an hypothesis, the calculation of pros and cons according to prior knowledge, in harmony with our interests at the time. We usually have explicit and implicit assumptions. Both will be found if we go back far enough,

[7] *Ibidem*, p. 444.

although on the surface the explicit ones suffice. In the words of one authority: "The verification of the explicit hypotheses requires an interpretation of experience subject to the implicit hypotheses. In their turn these latter are in great part verified by means of other theories and explicit hypotheses, and are corrected when needed, by a wider comparison with the knowledge already acquired." [8]

Association, as was recognized early in the history of inductive logic, plays a notable part in this act of hypothecation. It is unavoidable that men let themselves be guided somewhat by the past, no matter how much argues against it in the abstract. Association by similarity, apparent or real, exercises an appreciable influence because "thoughts which resemble one another involve brain processes which at some point have identical elements" or "identical nervous pathways." [9] This physiological view of a psychic fact may be accepted as a reasonable way of explaining the value of associational thinking in scientific research. But of course, purpose also directs our quest for premises, thus eliminating automatically certain rival assumptions which later one may prove to be superior. The results must decide in the matter. If the end justifies the means in the sense that inductively gathered data tally with conclusions deductively made, we may feel sure of both. We must however remember that a perceptual consequent studied in the laboratory may have many antecedents, and vice versa, so that on the one hand "causes" experimentally verified need not after all be

[8] Enriques, F., "Problems of Science," 1906 (Transl. by Mrs. K. Royce, 1914), p. 165-66; also Jevons, W. S., "Principles of Science," 3. edit., p. 228.

[9] Angell, J. R., "Introduction to Psychology," 1918, p. 165. A very lucid presentation also in Muensterberg, H., "Psychology, General and Applied," 1914, ch. 8.

irreducible units, while on the other hand such final units *must* be interrelated uniformly, however variable their perceptible quantitative relations. *If* we have found real indivisibles of events in a qualitative sense, proving them single events, their several inter-connections will constitute exact laws of nature. Such is the scientific view of a Uniformity of Nature.

Statistics as Second Method.—But suppose our inquiries do not reveal such absolute regularities of recurrence? Suppose our events are not as homogeneous as those of physics and chemistry? Is then a generalization impossible? Shall we then forego our desire to fathom the inward nature of things, to discover necessary connections and regularities of recurrence less than perfect?

The answer to such questions is the use of statistics and of a new type of measurements. Statistics as a method links the quantitative exactness of experimentation with the purely qualitative analysis of reflection. But it must be allowed, incidentally, that it cannot be more than a method. It cannot be a separate science, for the relations it analyzes form part of many fields of work, each of which is already recognized as a true science. The original meaning of the word statistics is not tenable to-day. Human relations have long been assigned to several distinct, albeit affiliated, studies; and the variety of data covered by each has enormously grown. What is more, there are also natural sciences which avail themselves of the statistical method, so that as a science statistics would be extremely eclectic, to say the least. Whether it be the study of games of chance, or of the weather, or of organic heredity or of the variations of morphological traits in

organisms, or of social data—in all cases the statistical method will prove useful regardless of what particular science is involved. As a method statistics must interest methodologists. As a study of some select group of events it can form merely a part of a larger problem preëmpted by this science or that.

The Field of Statistics.—What then are the circumstances which make a resort to statistics advisable or necessary?

In the first place, though, statistics is applicable also to certain data of the physical sciences, it refers chiefly to events or units which are secondary and complex. That is to say, the units are most commonly not the last, irreducible ones known to physicists or chemists, but such as are presented directly to the senses and built up, as it were, out of irreducible units. A reduction of perceptual units to the conceptual of abstract science is always possible. If we so choose, we may trace a definite qualitative or quantitative relation between the things of common sense and the atoms or electrons (or whatever the name be) of the scientist. But the statistician usually takes things without reducing them to final units. His events or propositions are secondary magnitudes whose components may be shown to observe physical laws. In this sense also the units of the statistician are commonly, though not always, compounds and groups of events. What in the preceding chapter were called event-complexes constitute a large portion of the statistical raw-material. The weather, e.g., is studied as a single fact, though it comprises a number of distinguishable facts such as water or vapor, temperature, air currents, etc. Each of these constituents is again resolvable into finer units, which

natural science analyzes for one purpose or another; but all of them together make up the class of events known as the weather. So also do deaths or wage-rates or organisms and their parts, or the categories of social sciences, represent complexes more or less apparent. Relations such as constitute an invalid or an epidemic or a crime or an improved-farm may be treated as single entities, though of course at a risk only too familiar to students.

In the second place, statistics is a valuable method where the "conditioning" phenomena are either indeterminate in number or of unknowable make-up, as for instance in the throwing of dice or in meteorological events. In such cases the classifiable number of *elements* may be few. But if time and place or spatial units affect our calculation of magnitudes, if the facts accompanying our sequence cannot be measured or qualitatively analyzed, we must treat them as variables and trust to numbers of occurrences for light on the problem. Even then no principle of constancy need appear. That would be a question by itself. But the mere incommensurability of our modifying factors for any particular set of events will urge us to try statistics.

In the third place, the temporal and spatial units in statistical measurements are frequently indefinite. Instead of close successions or of simultaneities evident to our senses we have vague sections of time and space. The total period or area for which our events are studied may be of uncertain length; and apart from this, the intervals between the events of any one *single series* may be ill-defined. A hard and fast line therefore cannot be drawn between sequences and co-existences. Frequencies and correlations will be treated independent of time, or as if

it made no difference whether they occurred together or in succession. Our temporal and spatial units, too, are likely to be larger than a natural scientist would commend. Units of seconds or hours are not common. Again, a certain license with concepts of periodicity is taken by statisticians, which colors their results. More important however: Large time units multiply the variety of possible combinations of primary elements into the event-complexes counted and compared by statistics. Regular connections thus are less easily established, and the principle of plurality of cause and effects has full sway.

This means then, in the fourth place, that statistical methods are serviceable whenever our recurrences of relations are imperfect and impermanent. Instability of frequencies is the other side of the stability which we find and accept as the utmost attainable in such fields. Perfect regularity in all details would mean a law of nature, and is attributable to facts which give experimentation its supreme position among methods. Partial repetition, and hence a variable degree of constancy, points to conditions demanding a statistical adding and subtracting. Variability is unavoidable because of the immense number of facts involved in our examination of particulars. Increasingly as we pass from inorganic to organic, and from biological to social, phenomena we meet with complex relations, with things inherently changeable and subject to erratic fluctuations. Viewed statistically the world is measurably in a constant flux. Facts never repeat themselves exactly as once experienced. Each episode is unique, and intertwined with others so as to defy our ingenuity to unravel them all. Variability and movement exist objectively. As metabolism or as an *élan vital*, as

human will or public policy this disposition to vary manifests itself in things of life. In lieu of a mechanical parallelogram of forces we have an incessant anabolis and katabolis of physical and psychic aspects. Change thus is more than quantitative. It is not only a variation of magnitudes that must be gauged, but also change of qualitative relations such as everybody observes on a limited scale, without special means and methods. Statistics for this reason centers in problems of *rates* of difference and frequency, in percentage scales and comparative studies of variables.

Finally, and fifth, it is a commonplace that the statistical method is most effective where the events to be studied cannot be reproduced at will. Vital phenomena, the shaking of dice, economic relations, and the data of bio-metrics are not amenable to laboratory measurements because we cannot isolate particulars, cannot recreate all the conditions accompanying each set of correlates. Hence, for lack of a deliberate predetermination of magnitudes or different classes, we must count them *as they come*, noting variations and establishing interdependencies in that manner. If we wish to make selections it must be conceptually rather than perceptually. In other words, while a chemist may detach real events in time and space, the statistician will detach them only by way of classification. He must classify and then count classes and their respective frequencies. To this extent he may single out certain happenings for his own purposes. But the actual happenings escape his control.

Statistical Measurement.—The chief branches of statistical measurement rest directly or indirectly on these five characteristics which delimit the field of statistics. It

will be convenient, however, to distinguish from the outset between its external technique and the principles of measurement as such. Or rather, we would do well also to differentiate sharply between a descriptive and an inferential statistics.[10] For it is one thing to measure things or to record relations, and quite another to infer anything from these as to future events. Measurements may be easy when induction is impossible!

The external or mechanical side of statistics is not, of course, negligible; but it may be stressed at the cost of principles which bear immediately upon problems of induction. To a degree the statistician is helped by calculating devices and the use of logarithms. He observes certain rules in collecting materials, in making out questionnaires and schedules. He should acquaint himself with the best methods of rounding off figures or smoothing his lines for visualizing results. There are questions in tabulation and in the summation of results, in the making of graphs and diagrams; and so on. Such matters deserve careful attention, especially if accuracy or inference is nowhere a vital issue. But what statistics is chiefly concerned with is measurement. The outstanding topics of a statistical treatise will always be units or classification, a counting of variation and frequency, the use of averages, the analysis of dispersion, and a correlation of events for inferential needs. When principles for these have been laid down, the further question of the validity of statistical induction has already been answered in large measure.

A definition of units is important for the same reason that makes it so important in all scientific work. We

[10] Keynes, J. M., "Treatise on Probability," ch. 27.

must have standards and classes for comparison. Scientific methods always turn on inclusion and exclusion. Classification invariably is the beginning as well as the end of researches. It may be indifferent to us whether we call our units for measurement objects or events or propositions. But there must be no doubt about the nature of the thing to be measured and correlated with other things. Consistency in definitions is essential. Uniformity of selection is a first guarantee of success for later comparisons. Yet an element of arbitrariness cannot be avoided. Though not all definitions are postulates, statistical units frequently are no more. What wages are, e.g., or what constitutes a clear sky, or what a death from cancer is, or what should be our definition of a "psychic trait"—these are questions answerable only by agreement, without reference to known correlations. Again, magnitudes lose continuity by being classed. Discreteness and continuity are always vexing factors when nicety of calculation is required. We may need composite classes such as index-numbers in measuring costs of living; or we may let averages serve as units whose composition can only be roughly homogeneous. In any case the definition of units is a consequential step, and this the more so since the counting of many of them may aggravate the evil of a faulty classification.

Counting however is necessary because of the variability of our data. Indeed, statistics might be defined as the counting of classes of events and their numerical comparison. While natural scientists may rely upon a single occurrence—as they have demonstrated again and again—statisticians must place their faith in a law of large numbers, in endless repetitions and the measurement

of relative differences among members of a class. In fact, they must count in two directions. For there are, first, magnitudes whose differences may be put into orderly array for one purpose, and there are, secondly, frequencies for each member of the line-up which may be wanted for quite another purpose. Measurement concerns both differences of magnitude for a given group of classes, and differences in the number of recurrences for each member of such a group. An incidental result of such variations is the need of interpolations and of a smoothing of curves, where the events are counted only in part, or where for one reason or another they do not exist. But the principal task will be the determination of *how much* must be counted for a proper diagnosis of fluctuations. What differences to ignore and which to include, and how to find the total frequency or frequencies for particular temporal and spatial units, that is the chief problem! The time element may complicate it appreciably, as the makers of index-numbers know. The choice between moving and fixed bases and averages may be as difficult as it will prove important in the end. Our notion of relative magnitudes and frequencies is materially affected by the *scope* of our measurements and by the choice of series of events happening in time. Histograms and ogives and historigrams therefore must be referred back to our definition of classes and principles of counting, especially where socio-economic phenomena are compared.

Averages [11] apparently simplify the situation, but at

[11] For recent statements on value of averages see Carver, H. C., in *Quarterly Publications of American Statistical Association*, 1921, p. 721. See also: Mitchell, W. C., in Bulletin 284 of Bureau of Labor Statistics, October, 1921; Zizek, F., "Statistical Averages" (transl. by Persons, W. M.), 1913.

last analysis they prove merely that we are satisfied with something less than the utmost possible accuracy. They are used because of the limitations of our eyes and of our mind, and on assumptions which cannot be demonstrated in the great majority of cases. For the average, as statisticians commonly construct it, is a condensation of fluctuations, or the elimination of minor fluctuations whose real share in the frequency of any one event may be different from what we believe. What may be called a bare numerical average is relatively insignificant in statistical work. To say that the average of the sum of three, five, and seven is five, means little if we have colorless magnitudes such as mathematics or formal deduction to manipulate. But if we count real things and events in the outside world, related to many other classes of events and modified by them from time to time, an averaging involves almost certainly a disregard of some facts. In the words of the logician: causal relations are misrepresented. The interconnections of each event or of each series of events per class are partly ignored, partly shifted in space and time. Some relations are magnified, others neglected. The irregularity of statistical relations and recurrences leaves no other conclusion. Statistical as *functional* averages, hence, are a makeshift whose advantages are often offset by weaknesses that are real, even though not measurable. Or to put the matter differently: With the exception of the median, averages are artifices. They are products of a creative mind. Like laws of nature they are compounds of something objective and something else that is strictly subjective. They are marred by the liability to error which characterizes all human acts. Nature knows individuals and relations, but it does not know

arithmetical or geometrical averages, or even the mode.

The limitations of the average are, to an extent, admitted by statisticians when they calculate the dispersion of frequencies and its co-efficients. For here we find one set of items in an array of magnitudes used to qualify our estimate of another, or to qualify the average itself which represents the whole group. Dispersion is of individual differences as well as of their relative frequencies. It amounts to an attempt to consider individuals in spite of the fact that large numbers are necessary for a discovery of regularities. We decide at first to lump inequalities, to ignore minor variations, but forthwith compare frequencies for sub-groups within a given series so as to obtain a clearer picture of the entire situation. Averages serve as a standard for measuring deviations which in their turn throw light on the true value of these means. Absolute and relative deviations thus have increasingly engaged the attention of students. Ideal curves of "error," i.e., of dispersion away from the type must be corrected by actualities that the Gaussian figure did not originally cover. Irregularities of diverse sorts remain to be ascertained according to the nature of our subject, and this skewness is expressed in terms of the difference between several sorts of averages found for our series of events. Quartiles and decils gain significance in localizing movements away from a standard distribution, while coefficients become valuable for practical applications such as insurance companies or economists desire. But in these refinements of measurement some "errors" must after all be overlooked, and their ultimate sources remain obscure. That is, coefficients of every

sort are makeshifts because they point back to definitions of average which are essentially subjective. Variations cannot be measured and correlated perfectly. Differentials in appreciable proportion escape our vigilance, particularly where time is a factor in our reckonings!

Statistical correlation, too, compares unfavorably with the results of natural science because of the range of variability of events. Yet it constitutes the main object of all measurements, as already shown. The great bulk of statistical inquiry aims at correlations of one class or another. In ordinary frequency measurements the correlations are implied rather than consciously sought; but of recent years the other kind—what may be termed specific correlations—have come in for their share of recognition.

All frequencies are akin to correlations because they refer to definite classes of events, each of which comprises in reality a number of things or other events. This follows from the secondary nature of our statistical units, from the fact that the units are compounds or event-complexes whose composition is partly unknown to us. In speaking therefore of a death-rate or of the turning-up of a certain number when throwing dice, or of the frequency of a given income or of the distribution of ages in a population we are necessarily establishing a correlation. We do not think of it as such chiefly because of our definition of the class. There is only one variable distinctly pointed out, and so the others are forgotten. In specific correlations, however, two or more specified variables are compared. They belong evidently, or so far as we know, to allied groups of relations—of what are usually styled causal relations. Hence correlation in

the narrow sense aims at a discovery of these common interdependencies. Their quantitative manifestations are studied. Reënforcing and counteracting events engage our attention and yield positive or negative correlations of varying degrees. *Qualitative* correlation concerns the degree of regularity with which sympathetic movements of two or more variables recur. *Quantitative* correlations show the ratios of variation for our variables, for which a "coefficient" may be found exactly as in the case of dispersion. But it must always be borne in mind that coefficients depend upon prior computations, definitions, and assumptions whose value is in part uncertain. To have measured exactly a degree of relative fluctuations and frequencies, in time or out of time, for two or more variables is not to have proven the correctness of the premises with which we started out. The very fact that we may adapt different kinds of coefficients to different uses, as we do with averages, should remind us of the purely descriptive function of such terms. Correlation as a description of numerical variations which are more or less proportionate is a safe procedure. The rub comes when we infer from such matters of record the course of similar future events!

Statistical Induction.—Statistical induction, as remarked before, must not be confused with descriptive statistics. The latter deals simply with measurements, and measurements for present purposes may be called viewpoints. We may vary our estimate of statistical events because they are studied collectively, in large numbers, and for the most part as *quantitative changes*. On this account several modes of measurement are permissible and net us different viewpoints of one and the same object

or situation. Successive measurements by different averages and coefficients, for different series of like events, correspond to a set of impressions we get by approaching a landscape from different angles. We cannot view the whole at once; neither can we reconcile all the features displayed in our several approaches. But as long as we content ourselves with what we do see, making no predictions, all is well. It is only when we infer from the present to the future that difficulties arise.

Before considering briefly, however, this inductive problem of statistics, let us first revert to two points of old standing. Let us remember that causality is not a fact distinct from regularity of connection, and that the process of inference is everywhere the same, whether we are business men, scientists, or philosophers.

Since causation is merely another word for regularities of recurrence, statistical regularity of frequency or correlation must be just as causal as laws of nature. Causes and effects are simply terms for specific antecedents and consequents or members in a coexistence. Nothing else. True however that absolute constancy of relations does not exist in the fields explored by statistics, and that to this extent causes or effects are only partally designated. And still more to the point: Connections must show a minimum of regularity before we shall attach any "causal" significance to it. That is, we shall not infer anything as to future recurrences until on the principle of Sufficient Reason, allowing for error, we feel justified in calling a certain frequency causal. Where this is not possible "chance" correlations may be said to exist which are not really causal. We might for instance notice the birthrate of a certain country to rise in the same proportion that its

paper currency expands. Or we might be struck with the fact that in dealing cards the tallest men got most of the aces. Such numerical correlations would by most people be called accidental, meaning non-causal. Coincidence or chance would be held responsible. But if we inquire seriously into the problem we shall of course learn that these correspondences between two or more assignable variables do not last. They do not repeat themselves; in other words, there is really no constancy of relations qualitatively or quantitatively viewed. Though we do not expect perfect regularity of recurrence, though we know that a virtually infinite "plurality of causes" prevents our finding all those antecedents which most commonly go with other specified consequents—yet our general experience prompts us to reject the above mentioned correlations as "causal." We grant that regularity is insufficient, and declare it to be so because of our ways of reasoning which underlie all our logic. The possibility of statistical causation however is not affected by this insistence upon a minimum percentage of constancy. The *complexity of our statistical units* explains why we are satisfied with *less* than perfect regularity. But in general, statistical relations are no less nor more truly "causal" than laws of nature.

Laws of nature indeed are nothing but correlations of a particular sort. "It is this conception of correlation between two occurrences embracing all relationships from absolute independence to complete dependence, which is the wider category by which we have to replace the old idea of causation." [12] The inherent mutability of organic and social events therefore accentuates the difficulty of

[12] Pearsons, K., "Grammar of Science," edit. of 1911, p. 157.

finding those unexceptional recurrences which are peculiar to strictly physical connections. Statistical units are not only complex. They relate also to processes which cannot be reduced to purely mechanical terms, to purely quantitative changes. Their dynamic character forbids it. We must abstract by ignoring classes of events as well as certain frequencies of occurrence. Time itself is a factor of utmost importance in tracing ultimate "causal" relations, for "everything in nature is apparently in a state of continuous change, so that what we call one 'event' turns out to be really a process. If this event is to cause another event, the two will have to be contiguous in time; for if there is any interval between them, something may happen during that interval to prevent the expected effect." [13] Specific causality indeed is regularly concealed from us in statistical measurements because of this time factor and our lax definition of it.

Inference however does not stop on that account. In all studies we must reason and employ principles which are the stock-in-trade of logicians. Statisticians too depend upon a routine of inference which is, in fundamentals, exactly like that of the laboratory student. Our measurements of differences and frequencies are guided from the start by certain definite purposes and assumptions. They may not be succinctly stated, but they exist. Our hypotheses furthermore are influenced by our previous knowledge of similar relations, or simply by associations of sound and symbol. We cannot do entirely without intuition. We must act on judgments for which no immediate justification may be at hand. We use the principles of Enumeration and of Substitution. We resort to

[13] Russell, B., "Analysis of Mind," 1921, p. 94.

reasoning from analogy, since all induction harks back to such comparisons and classifications according to resemblance and difference. We measure variables and their more or less constant relative changes with hopes of verifying our results now or later. Changes in the range of our series, in the counting of frequencies and in our definition of units are the outcome largely of such tests or a cross-reference. Whatever steps may be taken by the physicist in directing his researches and unifying his results, are proper also for the statistician. The difference between the two kinds of investigation does not lie in the inferential process itself, but in the varying degree of certainty attending their conclusions. Induction and deduction, hypothecation and verification, an imputation of causal values, and the rôle of intuitive insight—these are always the same. The grounds for generalization only differ.

Statistics consequently is intimately bound up with problems of probability. Aside from the psychological aspects of probability or expectation [14] there are mathematical and empirical-objective phases in statistical induction that must interest the social scientist fully as much as a mathematician. But let it be understood right here that at bottom it is a question of logic we are confronted with, not one of mathematical technique.

Mathematical theories of probability treat partly of social events, but not mainly so. À priori probability presupposes conditions which are not usually fulfilled in empirical statistics. It builds inferences deductively on à priori "knowledge of possible and favorable conditions."

[14] Keynes, "Probability," chs. 1-2. See also Jevons, W. S., "Principles of Science," 2d edit., p. 199, and Bode, B. H., "Outlines of Logic," 1910, p. 154.

It assumes that "all cases must be equally likely to occur," [15] and reckons with few classes of events as possibilities. Whether the à priori probabilities of causal complexes are equal or unequal, generally speaking the number of factors involved is very small. A postulate of Insufficient Reason or of Indifference may be invoked, but its force will depend upon the nature of materials used.

In the majority of cases, and especially in the study of socio-economic conditions, an empirical à posteriori type of induction is imperative. The final problem is: How closely do mathematical probabilities and statistical frequencies agree? Is the status quo, is past experience a key to future happenings and numerical constancies, or must all predictions be taken *cum grano salis magno?*

As is well known, statistical inquiry has proven the existence of a law which, to a gratifying degree, justifies some sort of generalization from known frequencies. A Law of Large Numbers or of the Stability of Statistical Frequency does manifest itself in most fields, so that treatises on probability have more and more gravitated about a few standard theorems developed during the eighteenth (and early nineteenth) century. The employment of samples as averages rests directly on this circumstance. We meet here with a new aspect of the principle of Sufficient Reason and arrive conversely at a Rule of Successions which says: "As the number of instances is increased, the probability that an event q is in the neighborhood of q' tends toward certainty; and hence, subject to certain specified conditions, if the frequency with which B accompanies A is found to be q' in a great number of

[15] Fisher, A., "Mathematical Theory of Probability" (transl. by Miss Dickson, Ch.), edit. of 1922, p. 18.

instances, the probability that A will be accompanied by B in any further instance is also approximately q'." [16] But is this inference necessarily safe? Do large numbers —however defined—guarantee recurrences in a high percentage of future cases? Does statistical induction approximate that of experimentation?

In general the answer might be stated in the words of a competent critic as follows: "To argue from the mere fact that a given event has occurred invariably in a thousand instances under observation, without any analysis of the circumstances accompanying the *individual* [italics mine] instances, that it is likely to occur invariably in future instances, is a feeble inductive argument because it takes no account of the Analogy" [17] binding such instances. In other words, though we may ground our inductions upon assumptions of a finite world and a finite number of possible combinations of events, whose relative weights and affinities are determinable,[18] yet the uncertainty of future empirical frequencies remains. Inference is risky, for one thing, because "where an effect is quantitative, and there are a number of contributory factors which one way or another influence its amount, fluctuations in these do not necessarily stand out in the results." [19] Again, it is risky because we are dealing usually with an indeterminate number of classes of events, and because our temporal and spatial units are frequently ill-defined. There are many probabilities, and the prob-

[16] Keynes, "Probability," p. 388.

[17] *Ibidem*, pp. 367, 392, and 111. For a similar view see Campbell, N. R., "Physics," 1920, ch. 7, and pp. 212-14.

[18] Keynes, "Probability," p. 258. See also Fisher, A., "Mathematical Theory of Probability," p. 172, and Sigwart, Ch., "Logik," 4th edit., vol. 2, pp. 706-07. All three references show attempts to find a final, logical basis for statistical induction.

[19] Joseph, "Logic," p. 558.

able values of the existence of events favorable to a second or third event differ materially. At any rate they are unknown. Again, our statistical series are for the most part heterograde in that individual events possess assigned attributes in varying degrees, besides being perhaps heterogeneous in other respects. Thus the assumption of equally possible cases is out of place. We might define them as "cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the specific event, are led to assume that no particular case will occur in preference to any other";[20] but little is gained thereby. It is here as with standard curves of error which were once held to dominate all kinds of relations. We shall find them often, but not always. Skewness of dispersion must also be taken care of. "The typical frequency curve in all vital, social, or economic statistics is always the binomial one; but it will require much investigation . . . to prove whether this supposition is right, or under what conditions the observations will show a tendency to the binomial law."[21]

The drift of statistical induction has therefore been strongly toward a study of individual series of events and their respective frequencies. Increasingly the principle of relevance and analogy has been honored in the formulation of statistical laws. Instead of ideal curves of error we discuss curves for particular classes of events, relative to particular temporal series and to averages selected beforehand. The necessity of subdividing large

[20] Fisher, A., "Mathematical Theory of Probability," p. 9.
[21] Westergaard, H., "Scope and Method of Statistics," in *Publications of American Statistical Association*, 1916-17, p. 251.

blocks of variations and frequencies has suggested itself. Lexian series have come to the fore and modified our earlier views of the value of mathematical probability. Thus the best method for inference is said to consist "in breaking up a statistical series, according to appropriate principles, into a number of sub-series with a view to analyzing and measuring, not merely the frequency of a given character over the aggregate series but the stability of this frequency amongst the sub-series. That is to say, the series as a whole is divided up by some principle of classification into a set of sub-series, and the fluctuations of the statistical frequency under examination between the various sub-series is then examined." [22] Concessions of a logical order are thus made frankly by men who pretend to statistical induction.

Limitations should be admitted the more freely since much depends upon averages and coefficients which are the commonest starting-point for generalizations. Averages may beg the question whenever they are functional rather than numerical. As a modern authority states the case: "If masses of items, which have evidently been variously influenced by quite independent causes, are taken together in a series, the average so computed has little scientific value since it does not express the activity of a unified complex of natural or social causes, and is as a rule poorly adapted to purposes of comparison." [23] Offhand this may seem a special problem in the construction of averages, but in reality it opens up the much larger question of statistical induction for any group of events. All

[22] Keynes, "Probability," p. 392.
[23] Zizek, F., "Statistical Averages" (Persons, W. M.), pp. 65, and 60-120.

measurements of deviations, all coefficients of dispersion and correlation,[24] all index-numbers or similar composites point to shortcomings that react adversely upon inference as to the future. Assumptions meet us everywhere. Sins of omission are probably greater than those of commission. We are careless of time-lags and minor quantities of variation. We rely upon large numbers when the definition of "large" is arbitrary. We classify events without making sure of their exact component correlates; and we ascribe virtues to ratios which are derivative rather than primary and securely founded.

Conclusion on Statistics.—In short, if a final estimate of the validity of statistical induction may at the present be ventured at all, it must be with the utmost caution. We should conclude that grounds for inference exist, and that its full value for social sciences has not yet been ascertained, but we should also emphasize its inferiority to experimental generalizations. On all counts statistics falls short of the standard set by the method of natural sciences.

Reflection as Third Method.—What was on another occasion called the method of Reflection gains therefore in importance, even though it seems at first thought very unsatisfactory. It must always be accorded a place in scientific work because the universe is more than a play of mechanical forces, and because the problem of values of all sorts differs radically from that of kinetics.

[24] A conservative view of the inferential value of coefficients of correlation, with special mention of the Pearsonian, is given by Bowley, A. L., "Elements of Statistics," pp. 316-25, and by Keynes, J. M., "Treatise on Probability," pp. 421-27. See also Boas, F., "The Coefficient of Correlation," in *Publication of American Statistical Association*, 1919-20, p. 683.

In reflection counting is subordinated to a correlation of values, and quantitative to qualitative analysis. We are still concerned about units or events, and we have again complexes of a high order before us, precisely as in statistics. But the success of our method does not depend upon fine measurements. Sometimes individual events will be evaluated, sometimes groups of them. For some purposes the magnitudes discovered by natural science or statistics will be revised because of intuitional judgments or an exercise of freedom that grants more to "moral" convictions than an exact reasoner can approve. Yet the usefulness of the method of reflection has been demonstrated sufficiently. It serves well wherever a quantitative analysis of relations is admittedly negligible or out of the question.

In historiography the description of particulars plays a notable rôle and with rare exceptions does not terminate in the formulation of laws. Granting these special cases, however, we do well to emphasize the *generalizing side of reflection.* For the most part we seek not merely "causal" relations, that is regularities of details, but also fundamental principles and systems of relations. Our material is taken from the living surroundings, from the sphere of hard facts which we ourselves, or others, gather for an evaluation. Reflection thus is more than reasoning of a formal sort, since our premises cannot be altogether arbitrary; nor can our conclusions stand irrespective of testimony to the contrary. Formal logic is not a field for reflection as here defined and discussed, however true it be that, loosely interpreted, reflection forms a part of all inquiry and of all sciences. It is facts that reflection is interested in, exactly as experimentation or statistics is.

Number and order as such do not provide the kind of problems that we can solve only by our third method of science. But whenever the conditions above mentioned are fulfilled, whenever the factual relations are to be treated qualitatively more than quantitatively, whenever the data directly before our senses do not constitute our subject matter, or at least only a portion of it, and whenever our units for correlation and generalization operate independently of the laws of change which basic natural sciences have brought to our attention—then a field is open for reflection.

Reflection, in any case, is not exempt from the routine of experimentation and statistics. Like them it also resorts to observation, even though much of it consists of memory and recall, aroused with or without stimulation from the outside world, and of records transmitted by others, culled from documents or books, or passed by word of mouth. Furthermore, like these more familiar methods, reflection involves comparison, analysis, classification, subtraction and addition by way of mental review, and a balancing of premises and conclusions not all of which find our approval in the end. Hypothecation, deduction and induction; allowances for error of fact or fancy; a rough gauging of magnitudes in so far as we picture them or give them numerical expression; a search for general denominators under which we may subsume all the data deemed relevant and weighty—all this is comprised in the third method which, while combining parts of experimentation and statistics, is yet different from them.

Creeds and viewpoints, to be sure, will determine the choice of our materials more than in the laboratory, particularly since verification by and to the senses is gen-

erally out of the question. But on the other hand, reflection calls for special qualifications the possession of which is a prerequisite to attainments of a high order. In the discharge of those duties which fall peculiarly to the man of reflection many traits must be highly developed whose value few might suspect. There must be breadth of knowledge and an excellent memory; a power for abstract thinking whose prime test is imagination and intuition paired with the ability to see differences and resemblances slight in degree and distributed over a large range of events; patient care in rehearsing facts and a conscientious regard for the happenings of the day, whether they be trifles or portentous signs and symbols. Whatever the problem we launch upon, in reflection we must display a truly scientific spirit or forfeit the chance of adding to knowledge. Theorizing may enter conspicuously into our work. Systems we may evolve and explain away as we please. But in essence our procedure must be like unto that of the natural scientist, lest great possibilities remain unexploited.

The possibilities *are* extraordinary because contrary to popular opinion the scope of experimental and statistical inquiries is narrowly circumscribed. The achievements of physics and chemistry have blinded us to the limitations of its technique; but that doesn't make them any the less real. Not all natural sciences rely exclusively or mainly upon the laboratory method. Geology and biology for instance must proceed largely by reflection, or relinquish the hope of ever understanding some most important questions; and so likewise psychologists. Wherever verification by the senses is impossible or quantitative measurement less helpful than a qualitative correlation of events,

there introspection has its place and will necessarily be accepted as a fair substitute for exacter knowledge. The philosophers in particular therefore have relied preëminently upon our third method, although social scientists have not been far behind them in this respect. And all things considered the results have not been meager. On the contrary, most of our current interpretations of the chief values of life have been attained by reflection coupled with statistical inquiries as a prelimniary or last step. Our view of reasoning and the nature of human knowledge, what we believe with regard to history and socio-economic processes, our systems of logic or of Marginism in economics, the leading doctrines of socialism and other reform movements, tenets in religion and ethics—all this and more springs from reflection as a distinct method for systematizing data and basing conclusions upon them. Its shortcomings of course are almost self-evident and not of a sort to be obviated by diligent application. We need never deny this. But none the less the virtues of reflection outweigh its vices. Used by a master mind it will produce results that have no superior even in the most fruitful of natural sciences: in physics and chemistry.

CHAPTER NINE

THE METHODOLOGY OF ECONOMICS

What is a Science?—If what has so far been said on the subject of inference, law and causation and the basic methods of science is in the main correct, the discussion of the methodology of some one science like economics may be kept short without impairing greatly its usefulness. It must follow from the general facts just stated what the scope and method of a special science is, or to be more precise, how we are to proceed in deciding upon its field, *modus operandi*, and worth as a pursuit of generalized knowledge. The content of general methodology, however distinctive in parts, must be in essence like that of any one specified science. It is only the fact that questions arise under a new name, as for instance whether economics is a science, or how it is related to other social studies, or what particular mode of measuring magnitudes is best adapted to it—it is only as these new queries arise that we are prone to think of each science having a methodology of its own, determined largely by a body of peculiar facts.

The question, e.g., whether economics is a science involves the broader one: What is a science anyhow? And here our answer may vary according to the rigor of our standard. Or we may candidly admit that definitions are often no more than an agreement without reason other than that of expediency.

Of course, that science is not merely knowledge, or an act of learning, has regularly been pointed out. We cannot allow the term "science" to be used so vaguely, if it is to serve any special purpose. We may grant that knowledge of many kinds is valuable and has been acquired according to principles clearly indicated and commendable to reason, but this of itself does not give us a science. More is involved than a mastery of facts or a routine of learning.

For the most part students have therefore sought to define science along two lines, stressing either the nature of the results, or the kind of methods used in obtaining them. Thus if we rely chiefly upon methods, it may occur to us that men are not scientific unless they proceed by experimentation, adopting the laboratory kind of measurement as the road to success. But would not this restrict the use of the term science unduly, to the exclusion of fields which have proven of utmost importance to us both theoretically and practically? For as has already been shown, there are not many sciences that can accomplish everything with the experimental method. Contrary to a popular belief there are so many other subjects not adapted to it that science and experimentation can hardly be considered synonymous.

In so far as *method* is any test, then, we must think either of the laboratory or the statistical principle of measurement in defining our term. Both kinds of method have given us valuable results, and both aim at similar ends, though it may not always seem so. Whether a careful reflection on facts, such as philosophers and many social scientists have practiced in developing their systems of thought, should likewise be a proof of scientific

work, may here be left in abeyance as a question that it is difficult to answer. For who can tell when reflection is sufficiently methodical, and when its results are in any degree comparable to those reached by the other methods? Evidently, no matter how conscientious our weighing of arguments and facts, or our review of the primary relations submitted by scientists, it will be next to impossible to guard our steps as carefully as may be done in employing the two other methods. At the best we could argue that science is a spirit as much as a procedure, and that in so far as men aim at truth regardless of consequences, judging without bias and holding themselves aloof from any temptation of personal advantage—that to this extent every thinker may be a scientist. Yet one may reasonably object to having the term science construed so liberally, as long as a far better test is available.

Thus, the most characteristic feature of scientific knowledge, and hence of science itself, is undoubtedly a body of theorems descriptive of permanent relations which provide a dependable ground for predictions or practical applications. Inquiries are scientific, from this standpoint of results, if they aim at a systematization of individual facts, ordering them into interdependencies that have objective validity, or at any rate seem to be real so far as circumstantial evidence of diverse kinds can justify such a belief. An inquiry is in effect a science, we may say, if it stresses correlations more than particulars, or if it connects these relations with an outside world of things and events of which our senses have some direct, primary knowledge. In so far as we make event-complexes and their constitutents, rather than abstracts of our imagination, the subject for examination, in so far

are we scientists if regularities of sequence or coexistence are discovered. The regularities may be absolute or relative; they may read like a first law of thermodynamics, or like a law of wages verifiable by a given amount of facts. In either case, and regardless of whether we have been experimentalists or statisticians, our claim to the title scientist should be considered strong. A scientific *spirit* may actuate many investigators. The powers of mind and of observation may be the very highest. But if our main body of facts is not focussed in laws that may be empirically tested, if description of individual data preponderates, and the conclusions are derived exclusively from explicit or implicit assumptions, our work is not, in an acceptable sense of the word, scientific. A science, in brief, is a body of knowledge organized into more or less verifiable generalizations or laws pertaining to physical or non-physical events, the determination of which depends almost entirely upon experiment or statistics. Mathematics consequently is not a science, albeit a field of measurement second to none in importance. Nor can the philosopher be called a scientist, however precious the results that he offers us.

True, however, that for most practical purposes it does not matter much whether we distinguish between science as here defined, and mathematics or philosophy or some other discipline. It is with the definition of the term science as with labels in general: If we know what's in the container the label is not necessary. All definitions are agreements, although the description they give us of the subject varies. The differences in the world about us are important, perhaps decisive for our weal and woe; the nomenclature we invent to indicate these differences, a de-

tail. Though economy urges us to coin words; though progress depends somewhat upon our symbols for identification and differentiation; though nothing is more attractive than a nicely worded, exhaustive definition in an argument—the chief concern of every student must be his understanding of differences or likenesses. If we know that one field of investigation differs from another in certain assignable respects, and what the consequences of these differences for other ends, our terminology is no more than a convenience.

On Social Science.—But granting that the term science is flexible, and that several tests for a science exist, what is to be said on the possibility of a social science, or of a science of economics?

Some economists, and notably German economists, as was shown elsewhere,[1] accepted the opinion of a leading group of philosophers that there could be no such thing as a social science, or at any rate that law and causation were impossible in the sphere of social events, since will and purpose created an unbridgeable chasm between the constants of physical nature and the variables of history. A distinction was made between realms where law reigned, and others where all events represented but the values of a feeling, planning, rational being. Between these two sets of facts a barrier was believed to exist, a barrier insurmountable and calculated to divide all pursuits of knowledge into two classes, viz., the ideographic and nomothetic. It was asserted, and repeated by some economists, that a scientist aimed at the establishment of types of events, while in the study of social events the particular necessarily absorbed our attention,

[1] See ch. 1 of this book.

making impossible the formulation of broad principles or of laws in the exact sense of the word.

What is to be said on this question, judging by facts previously considered?

In the first place clearly, we must agree to the distinction between physical and social laws. The eighteenth century attempt at uniting mind and matter for the purpose of extending Newtonian principles into the realm of psychics should not be pressed any further. It is evident from a variety of data that laws of social happenings, if they do exist, cannot be directly derived from the sort of associations which psychology studies, and for which a physiological explanation has, with some success, been offered. There is an indissoluble tie between mind and matter, but it does not allow us to identify the two, nor to stake our whole fortune on monism. Especially from a methodological standpoint is dualism an indispensable article of faith, a device by which we may hope to eliminate many of the errors characteristic of sensationalism, and without which human history can never assume a definite meaning.[2]

But in the second place, this does not commit us to any such classification of science as Voluntarists have favored, nor to the other idea closely allied with it that events are *either* a problem for historians *or* for scientists, but not for both. It is illogical to divide all investigations into the nomothetic and the ideographic, for it follows from the nature of law and causation that one and the same

[2] For a present-day statement of the difference between psychic and physical laws see Russell, B., "Analysis of Mind," 1921, p. 301. On the dialectic of social process see, e.g., Schiller, F. C. S., "Studies in Humanism," pp. 438-39, and Wundt, W., "Logik," edit. 1895, vol. II, p. 510. For a monistic evolutionary naturalism see Sellars, R. W., in *Monist,* April, 1921.

material may be studied either for the types of recurrences that it contains, or for the unique events which, apparently or really, owe nothing to law, and everything to will.

No data are intrinsically "scientific" or "historical," since knowledge is non-representative in an epistemological sense, a product of mind rather than an objective datum. What is known of the processes of feeling, perception, imagination and reasoning strengthens this belief. Laws may obtain anywhere, since every theorem presupposes a knower no less than something knowable. It is not a law here and an isolated fact there that creates a science of physics or an historical narrative, but our determination to view events from different angles, on the basis of certain assumptions, perhaps for specified purposes. Almost all objects and relations have a past, and hence lend themselves to an historical treatment. We may care about nothing else but this succession of individuals whose outer aspect shows continuity, and whose inner meaning is either determinism or free-will. However, we may also decide upon a search for regularities, of inter-relations that are as true to-day as yesterday, and as valid for the distant future as for the present. It will be for us to shape our inquiries accordingly, to select the data that do show such relations and degrees of constancy, and to hazard, on one ground or another, a calculation of probabilities. We may reject the associational theory of consciousness and of social events, and yet believe in the possibility of social laws. We may accept the doctrine of a human will which is at odds with the postulates of a mechanistic philosophy, and still be interested in a quest for social laws. Indeed, the statistical approach to

this subject has tended strongly to convince doubters. It is not dualism in any form that compels us to divide fields of research into two groups, but a particular variety of idealism whose zeal has overshot its mark.

Put differently: There is reason for drawing a line between natural and social sciences, broadly taken, but hardly a reason for imputing objectivity and subjectivity to different sets of events. What impels us to make a distinction between physical and psychic or possibly organic sciences is the undoubted fact that they represent two quite different kinds of units for correlation, for measurement. In the one group we have a few units definitely known, subject to observation with or without the aid of instruments, and measurable with a high degree of accuracy; in the other we have a much larger number of units about whose homogeneity we cannot be certain, but whose unfitness for experimental isolation and variation is beyond dispute. Thus we generalize about the first as we dare not about the second class of events. We have a feeling that law is real in the one case, and out of the question in the other. We talk of causation as if it inhered in the physical data, forgetting that cause and effect are names for items that constitute a law, and not anything else; or forgetting that all happenings are equally causal or non-causal according to our interpretation of terms. What differences exist between human and other events consist therefore not of the presence and absence of law, but of degrees of regularity and of definiteness of correlation, most social laws being in this sense "empirical," while physics or chemistry may expect to reduce all types of interactions to exact magnitudes and equations.

It is thus fair enough to speak of "tendencies" in economics and of rigid laws elsewhere, but we should understand, first, that the term does not mean an uncompleted action, and secondly that social laws represent in no wise a composition of forces, an average of arithmetical units, or a plus of psychics neutralized by a minus. The view which J. S. Mill took of empirical economic laws was colored by his belief in a mechanics of perception and ideation.[3] It made him hope that a science of ethics and sociology had but to wait for a sufficiently thorough study of the laws of feeling and thought, in order to rival the attainments of the inductive sciences.

Again, it follows from the nature of law and causation that economists must abandon all doctrines of imputation anent productivity and the price of services. Not only is it foolish to argue about the relative importance of agents operating jointly in the creation of a good or in the gratification of a want, but more especially must we reject an attempt at an ethical imputation. John Stuart Mill to be sure averred frankly that "when two conditions are equally necessary for producing the effect at all, it is unmeaning to say that so much is produced by one, and so much by the other. It is like attempting to decide which . . . of the factors five or six contributes most to the production of thirty."[4] Yet this opinion, which probably every economist would have subscribed to as an

[3] Find key to this in Mill, J. S., "Logic," Book III, ch. 10, §§ 5 and 8; Book VI, ch. 7, § 1, and ch. 9, §§ 1-3, and ch. 10, § 3.

[4] "Principles of Political Economy," Book I, ch. 1, § 3. See also Gide, Ch., and Rist, Ch. (transl. by R. Richards, pub. by D. C. Heath & Co.), "History of Economic Doctrines," p. 519. See also statement by Veblen, Th., in paper read before Kosmos Club, Univ. of Cal., 1908: "Causation is a fact of imputation, not of observation."

abstract proposition, did not deter many from laying down exact rules for finding the several individual parts of a joint product, or for assigning to specific parties so much of a share of income on the grounds of an imputed productivity. In questions of value no less than in questions of physical production the principle of ascription found a prominent place. What Menger and Wieser among the founders of Marginism prescribed as logical devices for distributive analysis elicited the favorable comment of later writers. The uselessness of the plan was not fully realized except occasionally à propos of an ethical treatise; and the chief reason for this unwillingness to abandon imputation was probably the view of causation inherited from the eighteenth century, and transmitted in modified form to later generations by J. S. Mill. Certainly in economics his influence was paramount.

But to pass over now to a weightier topic in things methodological.

How to Delimit the Scope of Sciences.—Economists have always been interested in a delimitation of their science, not merely because every scientist is likely to be, but also because of the nature of their subject matter. The relation between economics and other social sciences, or between either and psychology or ethics has regularly been discussed in the more pretentious European treatises. What then is to be our comment in the light of the leading facts of law and causation?

As regards the sciences in general it will occur to us first of all that the organic and inorganic fields may well be kept distinct, even though an exact definition of life is hard to give. The social sciences may also be sepa-

rated from the natural, not in the manner suggested by the Voluntarists of Germany of whom something has just been said, but on the ground that man is the center of all experiential data and himself their only interpreter. Furthermore, it is not unreasonable to arrange the fundamental sciences in a single progressive order, that is in a line beginning with the minimum number of concepts essential to research, and ending with the science which makes use of some of the basic concepts of all preceding sciences. We should thus have mathematics first, as dealing with spatial magnitudes and number, then physics which adds mass and motion in time, then chemistry dealing with elements and compounds in transformation, then biology as the field of life in forms lower than life and devoid of self-consciousness; then psychology which uses many of the ideas just mentioned, in addition to self-conscious behavior; and finally the social sciences treating of inter-individual human relations, inclusive perhaps of melioristic valuations.

Or we might essay a delimitation of sciences according to tangible objects studied, thus differentiating between astronomy, crystallography, botany, zoology, and geology. In a measure this principle would be satisfactory, provided we did not include all the sciences, nor think of special fields. For if we did we should notice that biology, e.g., embraces cytology, histology, bacteriology, and genetics; that the subject of man comprised psychology, history, and several social sciences, and that again histology covered botanical and zoölogical facts no less than those of human anatomy. Furthermore, we might remember the haziness of boundary lines at certain points between chemistry and physics, or biology and

psychology, to say nothing of other less simple disciplines. So after all the identification of each science with a particular class of concrete objects perceivable by our senses would prove embarrassing.

A better approach to the problem will be made if we start with an illustration something like this. Suppose we point to an oak tree and ask ourselves: What sciences, or how many sciences, have to deal with that object? We should then have to admit that such a single object may furnish food for thought to several kinds of investigators. Except that mathematics does not really treat of empirical facts, we could grant that a mathematician might use the tree for studying spatial relations of a certain class, say cylinders, cones, etc. A physicist would obtain possibly laws of gravity, light and color from it, or try to explain why the sap is able to rise against gravity. The chemist would have his compounds, their make-up and stages of metamorphosis; the biologist a set of growth facts for morphology and pathology, etc.; while an economist could discuss value and cost relative to soil and site, or problems of reproduction. In short, one and the same item—in this case the tree—would become the concern of a number of scientists.

What then is back of this significant fact? We must reply of course: Science studies relations rather than objects of common sense perception, and units following a mechanical law or expressable as functions of variables rather than things discrete in space. Each science selects types of units and of quantitative correlations, seeking as many instances of them as possible. We may conceive these units or groups of events as we please, describing them as seen directly by the eye, or analyzing them in the

manner of a chemist. But once we have chosen a certain class of units of inter-relations we shall be compelled by the facts to connect one instance with another, until gradually large numbers of laws or correlations are found, many of them to be subsumed under one general denominator such as force and motion or gravitation, energy, etc. For the natural sciences all laws may ultimately focus in a single concept such as electricity or matter-in-motion, so that somewhere the several originally distinct inquiries blend indistinguishably.

Other fields however will always be distinct, though commingling with one another at certain points. No hard and fast lines can be traced because of the flux and complexity of the units involved. Time and space units being vague or incalculable, variability being part of the units and of the event-complexes, while conditioning phenomena with respect to each correlation escape measurement, it follows that a definite territory for every science cannot be mapped out. It may seem so at first, and à priori such clearly marked bounds may be prescribed. But as the data increase and are being classified more and more nicely according to frequencies or degrees of regularity, such dogmatism falls into disrepute. Especially where mass-measurements are the rule our nominally distinct fields of inquiry will overlap in places, or coalesce in spite of the arguments of logicians. Thus changes in the socio-economic environment will not be without effect on the scope of the science in question. If old facts disappear and new ones rise to the surface, our correlations will have to be revised accordingly. It need not be imagined that the units of the organic and social world studied by the method of statistics or reflection will

change objectively, or enter into new, more or less permanent quantitative relations, without its reacting upon our sphere of inquiry. That is not likely.

The Scope of Economics.—Thus, to illustrate the principle before touching upon the relation of economics to psychology and ethics, suppose we assume a very different set of economic data from those now surrounding us. Suppose for instance all things useful to man were plentiful, so that no work need be done voluntarily. Or suppose absolutely everything were produced to be sold in the open market, or on the contrary that nothing were so exchanged. Or again suppose that prices for all goods and services were fixed by the government. Would the scope of the economist's inquiry, would the nature of his correlations, would the existence of his science be affected? The answer must be in the affirmative for the first of our questions, but negative for the last. That is to say, owing to new data coming upon the scene and the old ones disappearing our qualitative and quantitative formulæ both would look different, would be changed in composition, degree of regularity and perhaps permanency. We should have a new set of correlates and conditioning facts attending any one particular economic law. But economics as a whole would not be abolished by such substitutions of one régime for another. Some facts would remain as before. Men would still live by means of products and efforts. A residuum of activities would endure which could always be made the subject of a study to be known as economics—or anything else we like.

Hence it is not impossible or illogical to assign to an investigator a select group of data for analysis and reduction to types. We can always do this and pick out

any complex of events within our experience. But once we have given a name to a science of a particular type of facts, many additional data will belong to it and to no other science because of their factual measurable connection with the type of correlations selected. Thus we may decide upon a study of the facts of consumption and production, linking the first with biopsychological traits, and the second with the physical environment. The relations and regularities inherent in these two sets of facts can hardly be imagined outside of life itself, and hence leave a minimum for investigation regardless of what alterations are made in particular institutions. But how production and consumption are to be defined, what data in valuation, exchange, price and distribution or public control must be related with them—this is a distinct and subordinate question. Absolutely rigid limits cannot be set for a study of units and correlations as complex and unstable as those of human activities. There is no à priori ground on which we may condemn the exclusion of all non-exchange data. But neither can there be any objection from another viewpoint for extending our inquiries over much more than catallactics. If men must have goods furnished freely by nature or procured by effort, if the use of such things involves ownership and further legal rights, if at a given time production implies certain modes of living, valuations and central control, then these event-complexes may be indissolubly intermingled with phenomena of exchange. There is nothing for us to do but to find out what regularities are lodged in such varieties of interrelations, and then to state the scope of economics accordingly.

On account of the modern view of human nature the

catallactic analysis certainly is no longer in good odor. We are willing to acknowledge that man is more than a consumer or producer of scarce things, therefore plead for a broader conception of political economy. Yet it should not be supposed that psychology as a science is a prerequisite to, or logical basis of, economics; for that would be a lapse back into a methodology altogether out of keeping with our present knowledge. Not the science of psychology, but a certain fund of psychological data, will prove useful to economists. Psychological aspects undoubtedly form part of their field of research. But it would be fallacious to argue from these to a closer relation between the two disciplines. Indeed, in one sense all facts are psychological, and in a second there is nothing psychological but it is gleaned partly from other subjects. The professional psychologist himself relies largely upon economic data for expounding his theorems, and his obligations to modern sociology stand out strikingly enough. But we should bear in mind at the same time that psychology deals with the individual, relates body to mind in the individual, or mind with mind among different individuals as such, or is nothing but physiology. So it cannot very well be confused with economics which is interested in inter-individual relations regarding physical events and rights and forms of behavior foreign to psychology.

Neither should it be difficult to see a difference between sociology and economics,[5] although between these two

[5] On scope of sociology and its relation to economics there is a large, though chiefly periodical, literature. See among others the following: Durckheim, E., "Les Règles de la Méthode Sociologique," 3. edit., 1904, p. 157 ff.; Spencer, H., "Study of Sociology," 1873, chs. 4-6; Giddings, F. H., "Principles of Sociology," 1896, Book I;

the line of demarcation is even harder to draw than between economics and psychology. If we may judge by late developments in sociology, it links up more intimately with psychology than with economics, while on the other hand some topics are common to both sociology and economics. What is social psychology, sociology or psychology? That may be a baffling question. But we are safe in saying that sociology is to-day a better organized discipline than in Comte's age, and that economics is on the contrary not as sure of itself as a generation ago. What merits our attention chiefly is the strong tendency among sociologists toward qualitative analysis, while economists incline increasingly toward a quantitative analysis. That economists may learn much for their purposes from the sort of analysis exemplified in the latest sociological texts is quite certain; but that for this reason they should lose sight of their own peculiar realm is highly improbable. The sociologist may treat of all regularities or individual data within society, and so dare claim a very large field. Why not? But in so doing he is almost certain to touch upon facts which also concern the economist who studies a more specific and different type of correlation, namely one centering around facts of consumption and production. The complexity and variability of the units examined by social students is, once more, a major reason for a blurred boundary line between economics and sociology. Yet the overlapping will be harmless; nor can sociology be said to provide a

and Publications of Am. Sociological Society, December, 1920, pp. 60-67. Small, A. W., "Meaning of Social Science," 1910, and Lect. II; and article on "Future of Sociology," in Pub. of Am. Soc. Society, December, 1920, pp. 174-93; and his "Adam Smith and Modern Sociology," 1907, pp. 198, 200, 237, 77.

logical indispensable groundwork for economists. There will be cross-references, but not a progression from the more general problems of one science to the less general of the next.

The possibility and expediency of a distinction between the several social sciences suggests itself still more clearly with regard to the relation between economics and politics or history or ethics. But since the first two have always been sharply defined we need consider only the ethical problem, which has often proved vexing.

Economics and Ethics.—Economics was historically developed from ethics, and so it is perhaps no wonder that the question of the right relation between the two offered great difficulties, some deeming them worlds apart, while others felt them to be almost inextricably interwoven. It took centuries before the data of human life were effectually separated from Christian norms and moral judgments in general. As every economist knows, the divorce was not easily accomplished; nor was it at all certain at first that Adam Smith had broken resolutely with tradition. The Physiocrats undoubtedly treated economic facts as facts only, inspired by ideas that were taken over indirectly from the physicists and physiologists. They naturally made of human events an expression of physical laws. Adam Smith however must have found the difference between ethics and economics less momentous, and indeed made of his survey a theory of prosperity rather than a cold-blooded analysis of objective realities. So it was not until the beginning of the nineteenth century that economics was definitely sundered from all moral judgments, and raised to the rank of an independent science

that had a field as broad and yet clear-cut as any of the older disciplines.

Again, the nature of the problem of ethics and economics was rarely stated in definite terms. What at first had seemed to be an easy distinction, eventually was beclouded by a failure to differentiate properly between an *ethical judgment* of facts and the *origin* of such ethical judgments. Some believed economics to be a moral science because they wished their subject matter to conform to an ethical standard.[6] Merely for this reason did they make out of economics a moral discipline. Others more or less vaguely discerned a difference between economic facts and moral norms, but confused Applied Economics with ethics, presumably because of the purposive element in such applications.[7] A third group avoided these blunders, but only to make the mistake of deriving its norms systematically from social science. They understood apparently the issue, but identified human nature with the Ultimate Good in the manner of hedonists and the British empiricists. Now, what must be our stand to-day on this issue, considering the history of both of economics and of researches elsewhere?

As between the formalism of Kantian ethics and the content-ethics of other schools there is no doubt that the latter alone has so far met the test of inner consistency. Whenever the spirit of the thought or deed has been taken

[6] A view expressed by Paulsen, F., "System of Ethics" (translated by Thilly, F.), p. 4. See also: Dewey, J., and Tufts, J. H., "Ethics," Part II, chs. 22-4; Stuart, H. W., in "Creative Intelligence," a symposium by several writers, 1917, p. 352; and Small, A. W., "Meaning of Social Science," pp. 227-39. One is reminded of the Historical school of economists in this connection.

[7] "Sociology as Ethics," by Hayes, E. C., 1921; Ellwood, Ch. A., "Sociology in Its Psychological Aspects," 1912, p. 40; Bernard, L. L., in *Am. J. of Soc.*, 1919, pp. 298-325.

as the sole mark of goodness the question at issue has really been begged. A purely subjective norm of loyalty to duty or to conscience cannot carry the day unless we know beforehand that our sense of duty is of the right sort, our conscience an infallible oracle. To strive with all one's might toward the good, *as* intuited, is vain unless life itself is negligible or else the means toward its furtherance *given* in the very mandates of our inner voice. Only then would a realization of the law be the whole of virtue, compared to which other ideals shrink into insignificance.

But evidently this straight road to goodness cannot be taken by human beings who are not only weak-willed, but also devoid of the gifts which Kant and Spencer both credited them with. A formal ethics therefore must fall with a transcendental outlook. In fact, it has never succeeded even in the minds of its own sponsors, since sooner or later the question of *content* at least had to be answered. Precisely for this reason ethics is sure to have a body of norms which aim at a definite reality, at a set of circumstances or deeds or policies whose portrayal is the work of history and social science. Ethics must be empirical, not metaphysical. Whether we think of individualistic hedonism or of a social utilitarianism, or of any of the eudæmonistic systems so far evolved, in every case we must accept the experiential basis of our norms. The decisive feature of most ethical theories has been, on the one hand, its empirical tone, and on the other the stress of a purpose, of a goal of facts. The consequences cannot be ignored! The motive will impress us on occasions; but in the long run everything depends on the content which our behavior or our moral precepts give to life itself.

But where is this contents to come from? What decides whether an idea or an action is good or bad? Whence the Ultimate Good that is the keystone of the arch of every ethical theory?

In putting this question we are for the first time forced to admit that facts *as such* cannot yield a moral norm. There is no possible way of deriving an Ought from an Is. Whatever the data we work with in an ethical treatise, its highest norms will not be produced from these data in the manner of converting raw-materials into finished articles of economic value. The world of facts is not that of moral judgments. Or to state the situation more succinctly: A real chasm yawns, separating descriptive and normative analyses. Not that there are two groups of sciences, one descriptive of physical or social facts, and a second embracing rhetoric, logic, esthetics, and ethics. No, this time-honored classification is not a necessary corollary of our main thesis. But it will prove serviceable for the most part to put ethics in a class by itself, to realize that we have only two kinds of judgments, viz., judgment of facts and judgments of conduct. Many values there are, but we have only one *ethical* value. And that deals with conduct of man toward man or toward other animate beings, or toward a sublimated Self which is central in religion.

In short, we have to do with two entirely different viewpoints. On the one side is science which treats of events and their regularities; on the other ethics which considers man as a *willing* being. There the Is, and here the Ought. There a study of *responses* obeying fixed laws of nature, according to a mechanistic principle which science makes a postulate. Here a study of *responsibilities*

which rest on reason and power of self-direction. The scientist sees the world as a network of relations between objects and their changes. Constancy amidst change is the spectacle brought before us! But from a moral standpoint this constancy is but the proof of an absolute will, of a plan consciously made and carried into execution subject to the approval or disapproval of other responsible creatures.

In other words, to ask for the source of the Ought is to refer thereby to a master of events, to a captain of his fate, nay to a sovereign who rules supreme. Every Ought points to a master, as well as to a master key for creed or conduct. It rests with us, in an historical sense, whether our sovereign is to be human or divine, but there is no room for doubt as to the logical implication of an Absolute.

Until modern times, and especially during the Christian era, the supreme good was invariably embodied in, and attributed to, God. The source of moral standards was thought to be theistic, and the hierarchy accorded preeminence because of its superior understanding of this fact. The Church and the priest ruled undisputed. Revelation figured as the means of enlightenment on ethical problems. The Gospel represented these revelations, and conscience the inner voice by which the precepts of the divine will made themselves known to men. Ethics consequently was an offshoot of theology. Creeds became all-important. Tests of the Ultimate Good were, at bottom, subjective because covered by adherence to dogma; and absolutism remained the faith of moralists for centuries.

It is however possible, as later days have shown, to substitute a human for a divine will; to find a sovereign

in men among men; and to confess by the same token the relativity of moral norms, no matter how profoundly we are swayed by them. Thus, if to-day we ask what the origin of the highest Good must be, we shall do best to proceed quite empirically, studying the forces of minority or majority opinion. There is no way of finding out what is virtue in the abstract or in the concrete except by our consulting the views of the whole of society or of portions which decide for the remainder. If a minority sets up norms, it may be with the tacit assent of larger numbers; or we may find the majority dictate to the smaller group. It will depend upon the kind of norms we are thinking of whether they are ascribed to the will of the masses or of the classes. Generally speaking, though, the leadership of the few must be acknowledged, and in all cases the enforcement of what is declared right waits upon Might. That is to say, while from the standpoint of the (self-pronounced) righteous people certain acts may reflect nothing but Might, things cannot be proven right or good otherwise than by a dominance of opinion, by the physical or psychic control of either a numerical minority or majority. True however that so far in the human history the Ultimate Good has never aimed at anything less than the preservation of life. Acts of individuals and policies of groups that have been destructive of the whole human race have never yet passed as models of virtue. The foremost concern has always been the protection of life either on behalf of one person or of a larger group such as a modern nation, or of a still larger racial unit. That has been one outstanding element in all ethical systems. Life on the whole has been deemed worth while as a minimum to safeguard. Where one individual or

group has been enjoined to sacrifice its life, other lives have been understood to be gained in consequence. Whatever the disagreement on the right *contents* of life—and here the norms have gone far apart, as might be expected in view of the differences among men—the goodness of life itself has rarely been impugned.

So far as economists are interested in ethics, then, their position will be approximately this. *They will make facts a subject for ethical discussions, but not a source of ethical criteria.* This latter is exclusively the *human will* in its various manifestations and modes of self-assertion. Once men have decided upon the nature of the Ultimate Good, they themselves may be adjudged virtuous or vicious, and their thoughts or actions saintly or sinful. First the Good, then the Good Man! First the moral norm independent of facts, then the facts morally appraised in the light of our norms. Social data as such are a-moral. The principles of *economic* prosperity correspondingly lack a moral value. But *if* moral standards chosen by sovereigns of physical or psychic force, through a majority or minority, pronounce the conditions leading to economic prosperity moral, then—and then only!—may the economist offer advice of an ethical import to whosoever cares to use it. Economics in this sense waits on ethics, and not the other way round.

"Applied Economics."—Furthermore, it is at all times perfectly correct to apply economic principles irrespective of ethical standards. As long as we do not confuse such practical applications with ethics itself no theoretical difficulty arises except one. And that one is methodological in character rather than practical. Namely: In advocating public policies or private conduct, which shall

"apply" economic laws exactly as the natural scientist exploits his knowledge of the laws of nature, we are assuming that our socio-economic data are constants. At any rate, often this assumption is made. But to be consistent we should have to grant the change effected in our economic processes by this very act of application. The statesman cannot use the data of economics as the manufacturer may use those of a chemist. A difference exists which has already been explained under Law and Correlation, and elsewhere. A dialectic is continually going on between mind and matter, ideas and actions, creeds and conditions. It modifies not only the external appearances of economic life, but also the subject matter which we build into statistical frequencies, correlations, and qualitatively founded subsumptions. One need not preach freedom of the will in order to distinguish between the applications of natural and of social science. It is necessary only to remember the variability of organic events, and particularly of human stimuli and responses. The purely matter of fact view taken nowadays of the human will suffices to explain the conditional nature of "applied economics." We apply our knowledge, but thereby prepare the way for further applications of a different sort. Every action has a reaction that necessitates a new application. In this sense the Hegelian analysis is beyond reproach. In this sense all of us apply economics continually, the policies of parties and government being merely a special case which has excited our curiosity because of the large scale of operations involved. Yet applied economics must always be a variable among variables.

Statics—Dynamics.—What remains to be said further than this on economic methodology, is best introduced by

a brief consideration of the terms static and dynamic, especially since questions as to the scientific character and the scope of economics were almost from the start bound up with it.[8]

What the eighteenth century contributed to the evolution of this contrast was, of course, the conception of statics and dynamics in the world of mechanics. The facts of matter and motion were treated both as an equilibrium and as a differential giving motion to parts. The Newtonian system had revealed with great clearness the operation of opposing tendencies, and it cannot surprise us if students of the social order took a hint from this ruling principle, believing that what was true of physics would necessarily apply also to psychics. The identity of laws of nature and laws of mind had long been preached, not only by Greek philosophers, but also by psychologists from Thomas Hobbes on. English empiricism was a continuous apology for a mechanistic conception of human nature, the associationists being convinced of nothing so much as of the rigidity of the laws which determined the course of human learning or the succession of states of consciousness. Thus, without any express statement on the subject, most of the empiricists in England and in France took the dependence of mental upon material laws for granted. Monism was the dominant faith of the time, while dualism was in vogue only among the Rationalists who looked to Descartes and Leibniz for guidance.

Toward the end of the century J. Bentham, the chief protagonist of Utilitarianism on British soil, declared his Table of the Springs of Action[9] to be a system of

[8] For a history of the philosophical aspects see, e.g., Boucke, E. A. Goethe's "Weltanschauung," 1907.

[9] In his "Explanations."

"psychological dynamics." At the beginning of the next century Herbart in Germany launched his theory of psychic forces which, while designed to overthrow the older faculty psychology, rested none the less on a half mechanistic view of ideas. "The statics and mechanics of mind," he wrote in 1816, "deal with the calculation of an equilibrium and also of movements of our concepts." "Concepts," according to him, "become forces in that they counteract one another exactly like physical forces; and this happens whenever opposite ideas rise to consciousness." [10] Newtonian terms were thus transferred to the field of human events because it was felt that the divine order could not have been restricted to the planetary movements, or because there existed a strong belief that fundamentally human nature was constant. Given this fixity of human traits, and assuming as real the principle of Design or of Providence, it was not difficult to picture the world as the best possible, the prevalence of peace and order being a normal condition. Reason and justice, natural law and conscience, liberty and a Beneficent Deity—these were household words to the minds of the Enlightenment, articles of faith that most men would swear to as a matter of course. A static philosophy gathered strength in these stoic and naturalistic interpretations which agreed admirably with the settled conditions of the times.

The Industrial Revolution in England, however, and the tremendous upheaval in France, tended to give an impetus to another viewpoint which also had found friends

[10] Herbart, J. F., "Lehrbuch zur Psychologie," 1816 (Saemmtliche Werke, edit. of Hartenstein, G., vol. 5, pp. 15-6, 327-480; vol. 6, pp. 31-48).

here and there, and whose ultimate fruits were the doctrines of evolution and of Historism in many forms. Historiography had made great strides during the latter half of the eighteenth century. The interest in bygone ages which, though never dead, had flagged visibly before the Reformation, was powerfully stimulated by the practical political needs of that period. At the beginning of the nineteenth century the historical approach to human events had already been accepted as of primary importance. Philosophies of history and theories of progress were popular themes for scientists and poets. The metaphysicians in Germany had generalized upon the law of change and made out of it a logic underlying all processes of thought. Thus, from various quarters, the materials had been garnered that could serve social investigators well, if they had the necessary insight.

Now, Comte was one of those who believed in the regularity of human happenings, but was impressed also with the changes in time that historians made their exclusive subject. Comte, therefore, in addition to founding a science of "social physics" which should do for mental phenomena what physics had done for the world of matter and force, suggested that an analysis of the present be combined with a study of periods. Things as they are he wished to have studied as social statics, and successions of events, (analyzed with regard to their differences) as social dynamics. Or in his own words, "social dynamics studies the laws of succession, while social statics inquires into those of coexistence; so that the use of the first is to furnish the true theory of progress to political practice, while the second performs the same

service in regard to order." [11] This was the contrast that Comte deemed essential to a successful diagnosis of human affairs. With this admonition he gave to the world a concept that economists ere long adapted to their own ends, though not without introducing modifications which Comte, for one, could not have sanctioned; if for no other reason, because economics to him was not a science.

J. S. Mill must doubtless be given the credit for making a larger circle of economists acquainted with the Comtean idea.[12] However, even with him the central idea was still the buttressing of the deductive method by a use of what he called the Historical method. If Comte had made it clear that all things may be pictured either as at rest or as in motion, and if to the science of Order he had annexed the science of Progress, Mill as his ardent admirer not only accepted this contrast, but furthermore exploited it as a mode of reasoning that should rid the exclusively deductive approach of its foibles. For Mill dynamics meant no less the "necessity of connecting all our generalizations from history with the laws of human nature" [13] than the measurement of changes in invention, personal and property security, free-trade, and the extension of coöperation—of which he spoke specifi-

[11] "Positive Philosophy" (abridged translation of H. Martineau, 1855), p. 464. Similarly Ward, L. F., "Pure Sociology," 1903, p. 98. In general this idea is accepted by Mill, J. S., "Logic and Principles of Political Economy"; by Jennings, R., "Natural Elements of Political Economy," 1855, Preface; by Keynes, J. N., "Scope and Method of Political Economy," 1890, pp. 140-42; by Ward, L. F., in his "Dynamic Sociology." For a modified version see Spencer, H., in his "Reasons for Dissenting from Comte," March 12, 1864.

[12] See "Principles of Political Economy," 5. edit., vol. II, Book 4, ch. 1, § 1.

[13] "Logic," edit. 1871, Book 6, ch. 10, § 3 and § 6.

cally.[14] Or in other words, Mill was the first to detect in the distinction between statics and dynamics a methodological device supplementary to the rigidly deductive procedure which his psychology demanded. It was his wish chiefly to find data by which to test economic theorems based on the conception of an economic man.

Later writers continued to make a distinction between statics and dynamics, not merely because they had an illustrious example in Mill himself, but because the advent of Marginism meant both subjectivism and abstraction. The Utilitarian standpoint was, after all, objective, and hence likely to remind students of the eternal flux of life. In seeking to explain prices through costs or expenses, in laying some emphasis upon physical productivity, as well as upon principles of private and public budgeting, the Utilitarians were certain to deal largely with facts as presented to their senses. It was possible, as the records show, to adopt a risky simplification of human nature and of social processes, while nevertheless attentive to their environment. Marginists on the other hand turned a factual into a conceptual science because psychic interpretations displaced all physical standards, the need for simplification being now greater than ever. Statics therefore was contrasted with dynamics, and not only that: The abstraction involved in this differentiation was often forgotten, so that statics came to represent a normal state of affairs, while dynamics formed an ex-

[14] "Principles of Political Economy," 5. edit., Book IV, chs. 1-2. Two kinds of dynamic conditions are recognized by Pareto, V., "Manuel d'Economie Politique" (translated by Bonnet, A., 1909), p. 147 and ch. 3. See also Schumpeter's well-known views in his "Wesen und Hauptinhalt," 1908, and "Theorie der Wirtschaftlichen Entwicklung," 1912.

ceptional or at any rate incidental phase of the economic process.

On the one hand, then, economic data were spoken of commonly as forces which operated in certain describable ways and were either at rest or in motion. A static condition prevailed for the most part, according to this viewpoint. "It is even true," we read in one treatise, "that, as long as competition is free the most active societies conform most closely to their static models." [15]

On the other hand, statics meant a methodological device which reduced the bewildering number of actual relations to a comparatively few, thus enabling the economist to lay down precise rules for obtaining the best results. Instead of an indeterminate number of elements we get a determinate number.[16] Instead of wondering about the mysteries of causation we are frankly advised to rest content with a functional, virtually mathematical correlation of events. Instead of statistics, an experimental norm is introduced, it being held that varying the factors under investigation in the manner of natural science will yield exact knowledge of price and income. "Given an equilibrium for any one economic status, and a particular fact of interference with it, how will price and income be changed?" [17] This was stated to be the static problem par excellence.

Were then no changes to be reckoned with at all? Must statics mean a stationary condition, something like a body at rest all of whose parts are likewise motionless? The answer to this question was in the negative. An equi-

[15] Clark, J. B., "Essentials of Economic Theory," 1907, p. 195.
[16] Schumpeter, J., "Wesen und Hauptinhalt," p. 28.
[17] *Ibidem,* pp. 460, and 446-51.

librium, we read, is a "state which would be prolonged indefinitely in the absence of changes for conditions surrounding it"; [18] but this did not exclude changes of a quantitative kind which would modify magnitudes, without affecting the *number of elements* brought into correlation. At least this was a distinction frequently made, and to which those assented who gave the matter some thought. The difference between qualitative and quantitative changes was believed to be generic. For one writer the dynamic features were a growth of population, or of capital, or changes in methods of production, or of organization, or changes in consumers' wants.[19] Another writer reduced all dynamic agents to four, viz., 'variations of the extension of the zone of economic activity, variation in the relative amounts demanded by productive enterprises, for general and special outlays, variations in structure of population, and variations in those descending curves which represent gradations of costs of different increments of products." [20] A third author stressed changes in humanity (population, its wants and capacities), and in environment (land, capital-goods, and the loan-fund); [21] while a fourth mentioned as most important changes those in population, culture, natural resources, and the technique of production.[22]

For the most part the static condition was associated with an exchange-system whose study made of economics a science of catallactics. Changes occurring spontane-

[18] Pareto, V., "Manuel," ch. 3, § 22.

[19] Clark, J. B., "Essentials," pp. 203-06.

[20] Pantaleoni, M., in Publications of Am. Ec. Assoc., 1910, p. 112: "Phenomena of Dynamic Economics."

[21] Davenport, H. J., "Economics of Enterprise," 1913, pp. 453-54.

[22] Fetter, F. A., "Economic Principles," vol. I, pp. 400-01. For an interesting variant on these views of statics see Knight, P. A., "Risk, Uncertainty and Profit," 1921, Ch. 5.

ously within the economic régime were either considered unimportant, or of that quantitative type that affected in no wise the premises of the reasoner. For one group it seemed true that "the actual form of a highly dynamic society hovers relatively near to its static model, though it never conforms to it";[23] for another dynamics represented a transition stage that was to statics what the exception is to the rule.

Yet it may be regarded significant that the same writer who first said: "An equilibrium is a state in which, as long as no disturbing factor from outside appears, no leaning toward a change exists,"[24] added a few years later: "Economic systems would change even if nothing whatever underwent changes outside of them."[25] The need of a dynamic standpoint, in other words, was clearly recognized. It is admitted that, whatever the service of a static abstraction, "as a psychology of economic processes [it] is a failure in an important case, and can never be valid."[26] Not only were non-economic events tabulated as an integral part of dynamics continually at work, but what is more, the resort to dynamics for completing the economic picture gained popularity. The short- and long-time views of human interrelations were focused upon central themes such as hedonism versus energism,[27] or price versus valuation. Different classes of men seemed at the head of economic activities, dependent upon viewpoint; or the same men seemed to be actuated by different motives according to whether statics or dynamics

[23] Clark, J. B., "Essentials," p. 195.
[24] Schumpeter, J., "Wesen und Hauptinhalt," p. 36.
[25] Schumpeter, J., "Theorie der Wirtschaftlichen Entwicklung," pp. 469, 490.
[26] Schumpeter, "Wesen und Hauptinhalt," p. 512, Note.
[27] *Ibidem*, p. 128.

became the method of the economist. "The contrast," we are informed by an American observer, "may be put in general as the contrast between the theory of value, and the theory of price, statics being price-theory, and dynamics being value-theory." [28]

But even more room was made for dynamics by others who saw the danger of an excessive simplification of problems. If for one writer "there are as many 'static states' as there are economic problems worth studying," [29] for a second "any realistic study of distribution must be dynamic—," [30] while to a third a purely dynamic economics is the only satisfactory one. Thus as early as 1892 the preëminence of dynamic agencies is stated as follows, again by an American: "Changes in race psychology [i.e., "subjective qualities, desires, and feelings created in men by society"] give to men a new economic environment. This new environment modifies the standard of life through changes in consumption, and then the new standard acts upon the race psychology and creates new motives in production. This complete economy I would call a dynamic economy because it keeps up a series of progressive movements in society through the reactions between the subjective and objective worlds." [31] Historism and the economic interpretation of history thus helped to clarify men's notion of the dialectics of social development, the net outcome being a thoroughly dynamic version of economic types of events.

We are bound to ask: Is the distinction between statics

[28] Anderson, B. M., "Value of Money," p. 559. See also ch. 25.
[29] Knight, F. H., in *J. of Pol. Econ.*, 1921, p. 305.
[30] Ely, R. T., "Property and Contract," 1914, vol. I, p. 33.
[31] Patten, S. N., "Theory of Dynamic Economics," p. 38. See also the same writer's remarks in Publications of Am. Ec. Assoc., vol. 11, 1910, p. 128.

and dynamics to be upheld for future labors? Does statics rank properly as a "normal" state of affairs whose analysis constitutes the bulk of economic researches? Or may we turn our back on them, declaring economics to be a dynamic study solely, i.e., a study which is entitled to no more abstractions than natural scientists make use of?

Now the answer, it should not be difficult to guess, is the elimination of statics by studying things exactly as they are, irrespective of their intricacies. The sociologists have been a good example for the economist in this respect as in some others. They have not expatiated long on the pros and cons of statics, but instead adjusted their plans to the material directly before them. And this must be considered the only profitable method. For not only do socio-economic relations and conditions change continually—a fact most economists conceded in the abstract—but in addition these actual events differ qualitatively, i.e., *incomparably*, from those known to statics. The chief reason why economists cannot imitate physicists without invalidating their conclusions is their inability to estimate dynamic facts *quantitatively* after they have worked with statics. To claim: "The oscillations [of the price pendulum] are due to dynamic forces; and these can be measured, if we first know the nature of the static forces and the position to which, if they were acting alone, they would bring the pendulum" [32]—to say this is to promise the impossible. For, as we have seen, a radical difference exists between the units of natural sciences and those of social sciences. The first are regularly proven to be irreducible; they are definite and

[32] Clark, J. B., "Distribution of Wealth," 1899.

built into events that may be measured by rigid standards whose operation we may follow with our senses, with or without the aid of instruments. The second class of units however are as indefinite in many cases as they are numerous and liable to change both in an objective and in a subjective sense. The economist, unlike the natural scientist, does not deal with a demonstrably homogeneous class of things, except in so far as for any one situation he assumes a definite contents to make his measurements. Economic statics, consequently, cannot be to economic dynamics what physics can be to, say, meteorology; for the latter two deal with the same number of elements while the former two involve different numbers and kinds of elements. The meteorologist might predict the weather accurately at all times if he could measure all the variations in the magnitude of the few elements he is concerned with, these elements themselves being studied also by physicists. But if we wish to correlate social events as they occur we cannot count on the restriction of the number of factors that the advocates of statics demand. It is not merely a question of facing a vast range of fluctuations of elements defined for a correlation, but also of bringing facts into a formula in a dynamic view that the statical takes no cognizance of. Thus, while it is true that in part dynamics and statics so-called cover the same data in economics, and while again we must admit that causality is as real for human events as for the physical—supposing we accept the terms of causation at all—an "ideal" or static economics can not be made an index of actual dynamic conditions. Abstractions are part of scientific work, but they should not give us contradictory views of a subject. To

present the same data from several angles, at short and at long range with differences of emphasis or aspect, is one thing; but to alter the facts for the sake of a specious argument is still another thing. The first may be most instructive in its way; the second can only lead to absurdities.

The Methods of Economics.—Lastly, the *methods* of economics must be the same irrespective of the distance from which we view our materials. Nor do they call for lengthy elaboration at this point, since their several uses have already been considered in previous chapters. From what has been said on law and causation it follows that economists will resort almost entirely to statistics and reflection.

Experimentation is out of the question for the social sciences because we lack the means of isolation and reproduction, and cannot exactly measure the quantitative changes accompanying a particular variable. J. S. Mill himself was emphatic in making this clear, contrasting chemical with mechanical causation. As he saw it: "In social phenomena the composition of causes is the universal law." "The effect whch is produced in social phenomena by any complex set of circumstances amounts precisely to the sum of the effects of the circumstances taken singly," and "social science therefore is a deductive science——."[88] Now, this would hardly be an objection to the experimental method in economics to-day, since we cannot hold to the mechanical conception of human happenings entertained by Mill. So far from consciousness obeying the mechanical laws of association, as taught by the eighteenth century psychologists, they appear to

[88] "Logic," Book III, ch. 10, § 8 and Book VI, ch. 7.

us as synthetic products admitting of no summation. We should call the social laws chemical, and not physical, and thus disagree sharply with the sensationalists. But because of the indefiniteness of our units, because of their complex make-up and their instability, we are as keenly aware of the unfitness of experimentation for social science as Mill. We not only grant the plurality of causes, but also that of effects. We not only picture physiological processes in terms of chemistry, but likewise find it exemplified in streams of consciousness, in inter-individual activities. Thus we reject a proposal for laboratory methods in order to secure generalizations. It is obvious to us on first thought that events cannot be added and subtracted so as to leave a basis for comparisons. What attempts at experimentation may be urged by a would-be reformer, or by theorists offering a rule for action, will almost surely prove impracticable. We want none of these try-outs, and dread the useless waste and inconvenience to be occasioned by such a measure. Things are never exactly the same, we believe, because our whole life has been a chain of unique events in one sense. Thus the difference between the units of physical science and those of, e.g., economics is indirectly conceded. We may be eager to consult the facts and to verify our conclusions up to a maximum possible degree, but such inductions cannot obey the time-hallowed canons of agreement and disagreement.

In other words, we must proceed statistically if we are to undertake measurements at all. As has been shown, our units and their correlations may be handled in no other way. That regularities exist was evident to men a century ago, and that some of them attain nearly to the precision

of physical laws the Belgian Quetelet was among the first to demonstrate. All the characteristics that distinguish vital phenomena from all others also help us to decide when to apply statistical measurements and when the experimental. Thus we may choose for correlation perceptual objects, or larger event-complexes which themselves constitute relations between things or persons. We define and compare our units. We count frequencies and devise averages for convenience, ignoring fluctuations whose final explanation may not be given anyhow. We take care to make our classes or events comparable by all the tests which experience has gradually taught us to apply. We adjust our time and space units to the nature of our subject-matter and to the variations we may be measuring. Our series may have to be subdivided, and our coefficients of correlation be corrected in view of special known facts functioning as conditioning phenomena. There are many precautions to be taken lest our results become unreliable.

Yet if compiled with care statistics may be used both inductively and for purposes of verifying deductions directly from our knowledge of human nature. It is fair to forecast events on the strength of measurements regarding individual, or bundles of, events. Though our data will never be known as completely as those of a natural scientist, yet an agreement between hypothesis and our actual counts is a most favorable omen in many cases. We may assent to the dictum that "it is impossible to frame any general theories of value, interest, wages, rent, etc., by purely à posteriori method of reasoning";[84] but this will not blind us to the merits of statis-

[84] Keynes, J. N., "Scope and Method of Political Economy," p. 199.

tics, to the value of numbers, of frequency, of constants of fluctuation, of multiple correlations as a basis for short-time inferences. Results hitherto attained have not discouraged the investigator. The calculation of probability has become part of many a survey of facts economic and sociological. If statistics do not rival experimental methods in exactitude and magnificence of verified generalizations, neither has its method as yet been so highly perfected; nor have we had time to evaluate changes as integral portions of a cycle which—so far as we know—may repeat itself somewhat in the fashion of laws of nature.

Of the "Mathematical Method."—However, even apart from such shortcomings of the statistical method there is no denying that a great deal of the social scientist's work will always be done by reflection, that is with the aid of the third method which has been commonly called deductive, and for which mathematics has become famous. Not that the latter, incidentally speaking, has any distinct methods of its own, or brings us into touch with new basic principles. No. The mathematician reasons like other human beings. He relies upon premises and intuition. He hypothecates with the aid of known facts and under guidance of mental association. He deals with magnitudes and makes measurements by dint of close reasoning. He may correlate his chosen magnitudes and proclaim eternal verities. He may devise a language of his own and standardize his notation more easily perhaps than others. There are graphs and equations for him to develop that provide the ear-marks of a "mathematical method." But what really sets off this method from others is not the dress in which it appears, but rather its

independence of the facts of experience. If therefore we allude to a method characteristic of mathematics [35] we cannot mean something additional to the three standard methods of experimentation, statistics, and reflection. We can only stress the difference between a science drawing its premises from a factual world and being logically constrained to verify them (or else having its conclusions questioned), and another field in which conclusions relate to assumptions solely, not also to an environment of common sense. Mathematics may be called a unique discipline—if formal logic be not *one* with it—since it cares so little about empirical tests, and so much about rigorous thinking. The remark of a recent writer that "the perfection of the modern method [of geometry] is attained when it is entirely freed from dependence upon figures or constructs or any appeal to the perceptual character of space. When geometry is thus freed from this appeal to intuition or perception, the methods of proof are simply those which are independent of the nature of the subject matter of the science—that is, the methods of logic which are valid for any subject matter" [36]—this remark may well serve to differentiate mathematics in our mind from all other types of investigation. So far as this aloofness from content sensually derived is peculiar to mathematics it practices deduction and is in a class by itself.

But this being so, we must grant at the same time that economics cannot be simply deductive: for in economics we

[35] The economic mathematical literature of recent years is reviewed by Edgeworth, F. Y., in *Ec. J.*, 1908, vol. 17, pp. 221-32, 524-31; vol. 18, pp. 392-403, and 541-56. On use of graphs see, for instance, Waffenschmidt, W. G., in *Archiv. f. Sozialw. und Pol.*, 1914-15, pp. 438-81, and 795-818.

[36] Lewis, C. I., "Survey of Symbolic Logic," 1918, pp. 341 and 372.

treat of facts, and frequently of such as may be verified by our senses, even after they have been converted into scientific values. If then we use the phrase "deductive method," we must mean by it reflection as here understood. We must distinguish between the measurements of experimentation and statistics, and their absence in reflection. We must bear in mind the factual content of economics as against the conceptual nature of mathematical proofs. The employment of symbols and equations typical of mathematics will not make economics a mathematical subject, nor could economists on the other hand, who reduce their complex units and relations to a handful of magnitudes for purposes of coördination, be called anything but mathematicians. For surely, the mere circumstance that our entities are taken from an economic world does not leave them economic if their meanings and connections are destroyed.

But it is none the less right to accord a place to reflection in social inquiries, since qualitative correlations are as much a part of science as the quantitative. Whenever the determination of exact magnitudes is unimportant, whenever our regular recurrences relate to elements as such, simply as qualities or events, whenever types of relations and common attributes are sought that bind together large classes of seemingly independent relations, then the reflective method will take the place of statistics. There are many kinds of problems that no other method can solve. There is much reason in general for the sentiment, voiced by a sociologist, that "inspired intelligence" must always score heavily in the fathoming of truth, and that "the sooner we cease circumscribing and testing ourselves by the canons of physical and physiological science,

the better." [37] At any rate, in addition to statistics reflection must be relied upon for a correct evaluation of phenomena; and what is more, the time will probably never come when quantitative measurements can alone fill the needs of economics, or put to shame the results of qualitative correlation.

Reflection for economists will remain probably a mode of approach superior to statistics, no matter how much this latter accomplishes. Qualitative analysis may not be as intelligible to the casual reader as averages or a chart of coördinates well plotted, but as a subtler presentation of incommensurables, as a unified account of a complex process viewed from one angle, it should never cease to be attractive. If a rank must be assigned therefore to our principal economic methods, it surely will be reflection first, and statistics second as a tool for research. This seems just to both, and need not oust the statistician from his own peculiar sphere of usefulness.

[37] Cooley, Ch. H., "Social Process," 1918, pp. 397-400.

CHAPTER TEN

LINES OF RECONSTRUCTION

What to Discard.—In what follows some of the points will be seen to have been brought up before, or at least hinted at in connection with a discussion of allied topics. Others will here be added for the first time, partly because they may serve to indicate what changes seem most in accord with the suggestions of a host of writers during the last generation, and partly because it would be false modesty to subject the premises and principles of current economics to a candid criticism without taking the last step in which corollaries, theoretical and practical, are clearly stated. There is no harm in offering advice provided we do not assume a dogmatic tone, or reason on the assumption that it rests with one man or a few to point the way to salvation. What is intended here is not an unwavering declaration of independence which breaks nonchalantly with the past, with its achievements and memories of great men, but a revaluation of means and ends in harmony with current opinion, not only in economics, but just as well in other fields of inquiry. The trend of economic theories may not be what we expect. The extent to which a revision is feasible or advisable may be doubtful to all of us. But it is logical that we speculate on its probable course, and point out some of the changes of belief or emphasis upon which many appear to agree even now.

In matters of methodology there seems ground for rejecting the following, viz., first, the distinction between causal and functional relations in any science; secondly, the distinction between causation and law, except as aspects of one and the same situation; third, the distinction between law and correlation in the sense that one is causal, but not the other, or that only the first is of a scientific character; fourth, the division of the data of experience into ideographic and nomothetic, as if the existence or absence of laws could be demonstrated à priori; fifth, the idea that certainty inheres in physical processes, but is impossible elsewhere, so that experimental induction yields infallible generalizations, while statistical induction is altogether untrustworthy; sixth, the sharp separation of induction and deduction, and the designation of economics as a deductive science with a purely conceptual basis; seventh, the derivation of social laws of any kind from psychology or physiology; eighth, the associational-hedonistic theory of valuation and motivation; ninth, the recognition of two kinds of economic laws, the static and the dynamic, with the implication that the former is either self-sufficient or a necessary adjunct of economic research; and tenth, the belief in universally true quantitative laws of economics.

These are articles of faith that cannot be considered tenable at the present time, and the errors of which have already been brought up for discussion. But others now deserve special stress.

Thus we must disapprove of the identification of shares (incomes) with prices, or of reducing all classes of incomes to four, named wages, profits, interest, and rent. For the derivation of laws of price from a study of human

nature must be held impossible; and with this recognition of the difference between problems of economics and those of psychology will come also a different conception of laws of price, respectively incomes. Supply and demand, to be sure, will still figure as psychic facts whose significance for economists is far from negligible, but in trying to determine prices of goods and services we shall relate the physical view of supply and the pecuniary measurement of demand to the valuation aspects which heretofore have been given undivided attention, especially by Marginists.

Utilitarians, that is the friends of an objective view of exchange values and productivities, will have to abandon the hope of explaining prices by a deductive method even while clinging to expenses as a distinct category of determinants. Instead of this statistical measurements may render important service, unless indeed the quantitative analysis of social correlations is to be displaced entirely by a qualitative one. But this is a point not to be settled in a jiffy.

Marginists in particular will be hard hit by their consistent exploitation of a relatively few premises taken over from the eighteenth and early nineteenth centuries. Since the sensationalistic doctrine of valuation, e.g., has been thrown into the discard, they must give up also the hope, not only of explaining prices psychologically, but of establishing a more or less definite ratio between volume and value. In the past this reliance upon principles of utility has side-tracked economists in their quest for laws, besides causing some of them to confuse the physical aspects of production or value with the psychic, so that a logical impasse seemed to have been bridged when in reality

it was past mending. Thus purchasing power, output of things and services, and physiological states during or immediately after consumption were often enough confounded with psychic data that alone, by previous proclamation, concerned the Marginist. This blunder will hereafter be impossible. The enlistment of psychologists in the service of economics, while continuing in some form, will have a very different purpose.

But furthermore, Marginists will have no reason to retain the margin as a standard for measuring differentials, seeing that their psychological premises have fallen into disrepute. Measurement will become more important than ever but it will not be of a psychological character. Nor will there be any grounds for accepting a theory of imputation, either as a causal or as an ethical fact, in order to find thereby the components of joint-value or the specific contributions made by an individual agent of production. Whatever worth will attach to an allocation of values, it will not be logical; nor should it give rise to moralizing dissertations on labor and capital.

What is to be Retained.—As against what is to be excluded, however, economists will doubtless retain many of the features common to both Utilitarianism and Marginism. Now, among these the following deserve special mention, namely first, the acceptance of economics as a science; secondly, some of the principles or laws which so far have been discovered, and which are perfectly compatible with the change of front urged in regard to methodology; third, the recognition of reflection as a distinct method, whose results must always constitute a large part of economic truths; fourth, the admission of descriptive matter, even though it contain no generalizations

whatsoever, nor apply to anything but the facts of a given régime or locality; fifth, a logical unification of the principal divisions of economics, first because the events themselves are so inextricably interlaced, and furthermore, on account of the importance of valuation in any science like economics; sixth, the combination of a short-time with a long-time viewpoint, both however to take facts as they are, thus implying in no wise a return to a static abstraction; and finally, the rigid, unqualified exclusion of ethics from economics, even when it is felt that practical advice of any sort should be preceded by a confession of moral ideals.

New Problems for Present-Day Economics.—Not only must these elements of economics be deemed an indispensable part of future systems, but what is more, we shall have to prepare for certain departures.

In general, namely, there will be need of a purely dynamic viewpoint, which aims at a quantitative correlation of as many economic data as, in the course of our studies, prove to be fit for such treatment. Statistics therefore will play a larger, not a smaller, part in the economics to come, and this will tend to supplement the qualitative analysis of the past with another set of inferences now practically unknown. In the next place, economics should be regarded, not as a catallactics or plutology, but as the systematic study of all facts bearing upon relations of weal and wealth, whether this concerns scarce or plentiful goods. The pecuniary aspects of production, distribution, and consumption will thus give way to, or more likely be supplemented by, a tracing of sequences (or coexistences) that acquaint people with social interests and things, rather than with competitive

norms and counters of currency. And finally, the logic of our science as well as the trend of public affairs will compel economists to stress the national viewpoint, to point out succinctly the consequences of public control. Hence, in place of an abstract science of economics there will reign a political economy anchored largely in laws of cosmic force, but carrying a super-structure of generalizations mainly national in scope.

Such will be the principal innovations in keeping with the future trend of economic theory; but certain details may, tentatively, here be added.

Under the heading of Production, which should be given first consideration because of the primacy of environment and its independence of human valuations, the following topics will engage our attention: Essentials of human nature and its modification, respectively capacity for modifications, by the learning process in many individual and social aspects; the physical environment, natural resources, and the chief facts of national demography; capital as goods for production in the technological sense, and the make-up of wealth in general, physical volume of classes of goods and of services being studied as well as their relation to values according to competitive or collectivistic norms; labor forces as dependent upon population data and upon educational facilities, and the bearing of both upon supplies of labor kinds relative to ideals of income; the organization of the productive process notably in three phases, viz., the technical, legal, and financial, and the place of entrepreneur or government in the system now in force; principles determining national productiveness both from the short-time and long-time viewpoint; the relation between production and domestic

and foreign trade; and the ways and standards for measuring productivity in the non-pecuniary sense, with due regard for its applicability to distributive questions.

These will be topics figuring prominently in any analysis of production.

Under Price, as a second division in economics, may fall such outstanding facts as: The valuation process in its non-monetary aspects as preliminary to a qualitative analysis of price; in the next place the determination of laws of price—if they obtain at all!—by a correlation of price with a variety of events, and especially with: other prices of goods and services studied individually or by groups, with personal incomes or income-classes, with costs of the non-pecuniary, physical sort, with pecuniary expenses, where possible, with physical supply of goods or laborers, etc., and with data of foreign trade.

These data will be consulted as possibilities for discovering laws of price, apart from the qualitative analysis which economics hitherto has used almost exclusively. But there remain as further points for correlation—to mention only a few: ranges of price fluctuation relative to income changes; movements of wholesale as against retail prices; the territorial extent of a given price at a given time; price-level movements in point of order, degree of change, and direction, whether compared directly with currency changes or not; and finally the study of wages, rent, and interest in their bearing on facts of production and commodity prices, etc.

In the third place income should be studied independent of prices for the loan of capital or the lease of land, or even of the share assigned to labor, these incomes being measured per individual or classes of people instead of

figuring as prices paid for services in the open market. For prices acknowledgedly constitute only a part of the sources to which most people may look for income. From this angle, then, incomes may reveal principles not derivable from prices of any kind, while on the other hand prices may throw light on some or on all incomes.

Fourth: The problem of the growth (or shrinkage) of wealth will be partially solved by inquiry into modes of consumption, into profit rates and investment trends, into the processes by which thrift becomes national prosperity, and into the facts which may, or may not, be proven to result in business cycles, these cycles having a non-competitive no less than a competitive interpretation.

In the fifth place, public control is bound to receive much attention in future economic treatises, their revision affecting probably most of all theories of taxation, incidence and ability to pay, but also trading policies among nations. Not only this, but in addition new applications may prove feasible, be it price-fixation, or social insurance, or vocational control, or regulation of investment, or still other fields of enterprise which now are, in the main, a matter of freedom of contract. In these and further experiments the economist may wish to be able to offer advice. Whether he shall feel free to do so will depend upon his approach to economic data; but that in some degree legislators will continue to utilize economic research seems certain. As a science economics may not fulfill the promises once made with a light heart, but as an intensive study of national facts, with a stress on regularities that in a non-psychical sense obtain over wide areas and for long stretches of time (however variable by another standard!), economics may yield important re-

sults, offsetting by its usefulness to statesmen what it lacks in the universality of its truths or pretensions.

The Outlook in the United States.—Particularly in the United States, it would appear, this hopeful outlook is amply justified notwithstanding the dissensions among the economists for the moment. For here nearly all the conditions exist that are conducive to a rapid and original development of social science. A block of resources nowhere equaled in the world is ours, and bids us to further effort on a heroic scale. The population is sufficiently unified and organized technically to realize its opportunities and perform its duties. Pressing needs have sprung up since the World War, yet without their jeopardizing our national existence or annulling earlier endeavor. Education is being popularized and made to serve the interests of the masses as never before. Instead of idleness, it is labor for and with others which increasingly earns praise and tangible reward. Intensification everywhere is the watchword, and with it a deepening of the social conscience is taking place which will realign political and economic forces. Solidarity thus assumes a new meaning. Efficiency is subordinated to ideals. Enterprise has new regulations to observe, but in the long run is likely to benefit by them, besides improving the fortunes of the average man.

Economic legislation and instruction will accordingly become more rather than less important. The demand will be for persons who are trained in matters economic and know how to distinguish between individual and social norms. In high schools and in colleges the economic approach to life values will be increasingly respected and the cause of economic research gain in proportion. The

days when economics could be considered a set of theories which might challenge the speculative powers of a few closet-philosophers, but could have no further interest, are probably over. In an ever-widening circle economic topics are made a subject of serious discussion, nay, the concern of men who are chiefly responsible for national prosperity and progress.

All this then means that economists will have to establish, with much care, a broad basis on which to erect their edifice of generalizations and practical counsel. If it be not without significance that economics is the offspring of philosophy and psychology, neither should it be hard to comprehend that to-day a thorough drill in the fundamentals of valuation, in logic, ethics, epistemology, and psychology is an excellent preparation for, and asset of, the student of economic problems. A sharper distinction between economics as a science or philosophy of life and the so-called applied branches of economics may therefore prove beneficial to all parties concerned, and this not only because the two differ in aims or practical value, but also because of differences in mental attitude and prerequisites for success. For in the end it must be admitted —since the whole history of thought is testimony to our assertion—that specialization gains at the expense of a certain spiritual aloofness which characterizes science pure and simple.

It is for economists everywhere to decide whether they wish speedy results or an outlook that educates by degrees. But whatever their decision, can there be doubt as to the opportunities for service?

BIBLIOGRAPHY

INTRODUCTORY NOTE

The following points should be noted in scanning this list of references:

(1) The bibliography is subjective rather than objective. That is, it indicates roughly what kinds of literature helped to shape the present writer's viewpoint. No attempt is made here to give a complete account of readings which extend over many years; nor can the list be considered in any measure indicative of even the most important works relating to the several subjects. Much of what is best is not mentioned, while other works of secondary significance are mentioned because of the hints they furnished the writer. In the main, then, this bibliography is a *sample* of available materials for a restatement of the methodological problem in economics. As such, and only as such, it is offered.

(2) No mention is made of any literature on the *history or theory* of economics. The writer has tried to acquaint himself thoroughly with it, and especially with publications of the last two decades. However, it was felt on the one hand that a list of such writings is of no great value in a survey that is chiefly methodological; and on the other hand many references are given in the foot-notes of the first five chapters of this book.

(3) A large *periodical* literature on *non-economic* subjects has also been consulted, but with one exception is not here itemized. An exception was made in the case of psychological journals because of the data they furnished for a refutation of hedonistic associationism.

(4) The great bulk of economic methodology both before and since 1900 is German-Austrian. Indeed, it equals in volume that of all other countries combined. A perusal of the journals listed below will incidentally lead to this conclusion, although the main purpose is of course to show what periodicals were systematically gone over, in order to secure light on certain questions.

(5) The principal fields of literature, and the problems they were brought to bear on, are as follows:

Classes of Literature	Made to Bear on Problems of
Psychology	Valuation
Epistemology	Inductive Reasoning
	Nature of Science: Law
	Causation and Correlation
	Interrelation of Sciences
	Statics and Dynamics
Logic	Nature of Deduction
	Mathematical Method
	Proof and Probability
Methodology	Methods of Science: Measurement
	Scope and Limits of Statistical Induction
	Scope of Economics
Ethics	Relation of Ethics to Economics

A. PERIODICALS USED

I. The Social Sciences

1. *German*

For the Years

Archiv für Sozialwissenschaft und Sozialpolitik 1900–18
Jahrbücher für National-Œkonomie und Statistik (Conrad's) 1900–19
Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft (Schmoller's) 1900–18
Zeitschrift für Sozialwissenschaft 1898–1915
Zeitschrift für die Gesammte Staatswissenschaft 1900–15
Zeitschrift für Volkswirtschaft, Sozial-politik und Verwoltung, Wien 1901–15

2. *French and Italian*

Journal des Economistes 1900–12
Revue d'Economie Politique 1900–19
Giornale degli Economisti 1914–19

3. *British and American*

The Economic Journal 1900–19
The Economic Review 1900–15
American Economic Review 1910–21
Annals of the American Academy of Political and Social Science 1900–20
Journal of Political Economy 1900–20
Political Science Quarterly 1900–20
Quarterly Journal of Economics 1900–20
American Journal of Sociology 1900–21
American Statistical Association, Quarterly Publications 1900–1920

II. Psychology and Philosophy

1. *German*

Archiv für Systematische Philosphie.....1895–1917
Vierteljahrs-schrift für Wissenschaftliche Philosophie und Soziologie.....1903–18

2. *British and American*

British Journal of Psychology.....1904–10 (9 vol.)
Mind 1901–18
Journal of Philosophy (Psychology and Scientific Methods) 1904–18
American Journal of Psychology.....1900–18
Psychological Review 1904–18
Psychological Bulletin (Literary Section of Psych. Review) 1904–18

B. BOOKS

I. Psychology

1. *History*

Klemm, O., "History of Psychology" (transl. by Wilm, E. C. & Pintner, R.), 1914.

Baldwin, J. M., "History of Psychology," 2 vols., 1913.

Moore, J. S., "Foundations of Psychology," 1921 (largely historical).

Warren, H. C., "History of Association Psychology," 1921.

2. *Associational Psychology*

Mill, Jas., "Analysis of the Phenomena of the Human Mind," edit. of J. S. Mill and Bain, A., 1869.

Brown, Th., "Lectures on Philosophy of Human Mind," edit. of 1830 and 1854.

Bain, A., "The Senses and the Intellect," edit. of 1872.

Herbart, J. F., "Lehrbuch zur Psychologie," 1816 (edit. of Hartenstein, G., Saemmtliche Werke, vol. 5).

3. *Modern General Texts*

JODL, F., "Lehrbuch der Psychologie," edit. of 1916.
LIPPS, TH., "Leitfaden der Psychologie," 1906.
WITASEK, ST., "Grundlinien der Psychologie," 1908.
ANGELL, J. R., "Introduction to Psychology," 1918.
CALKINS, M. W. (Miss), "First Book in Psychology," 4. edit. of 1914.
DUNLAP, K., "A System of Psychology," 1912.
DUNLAP, K., "Outlines of Psychobiology," 1917.
HUNTER, W. S., "General Psychology," 1919.
MUENSTERBERG, H., "Psychology, General and Applied," 1914.
PILLSBURY, W. B., "Fundamentals of Psychology," 1916.
RUSSELL, B., "The Analysis of Mind," 1921.
TANSLEY, A. G., "The New Psychology and Its Relation to Life," 1920.
TITCHENER, E. B., "A Textbook on Psychology," 1910.
WARD, JAS., "Psychological Principles," 1920.
WARREN, H. C., "Human Psychology," 1920.
WATSON, J. B., "Psychology from the Standpoint of a Behaviorist," 1919.

4. *Psycho-Physics*

FECHNER, G., "Elemente der Psycho-Physik," 3. edit., 1860.
FECHNER, G., "Revision der Haupt-Probleme der Psycho-Physik," 1882.
MUELLER, G. E., "Grundlegung der Psycho-Physik," 1878.

5. *Abnormal Psychology*

FREUD, S., "General Introduction to Psycho-Analysis" (transl. by G. St. Hall), 1920.
GODDARD, H. H., "Psychology of the Normal and Abnormal," 1919.
SIDIS, B., "Foundations of Normal and Abnormal Psychology," 1914.

6. *Instincts and Emotions*

STORRING, G., "Psychologie des Menschlichen Gefühlslebens," 1916.

LIPPS, TH., "Vom Fühlen, Wollen, und Denken," 1907 (Psychologische Untersuchungen, 2 vols., 1912).

BARRETT, E. B., "Motive-Force and Motivation-Tracks," 1911.

BERNARD, L. L., "The Misuse of Instinct in Social Sciences," in Psychological Review, 1921.

CRILE, G. W., "Origin and Nature of the Emotions," 1915.

DREVER, JAS., "Instinct in Man," 1917.

FARIS, E., "Are Instincts Data or Hypotheses?" in American Journal of Sociology, 1921.

HOCKING, W. E., "The Dilemma in the Conception of Instincts," in Journal of Abnormal and Social Psychology, 1921.

HUNTER, W. S., "Modification of Instincts from Standpoint of Social Psychology," in Psychological Review, 1920.

KANTOR, J., "A Naturalistic Description of the Emotions," in Psychological Review, 1921.

MCDOUGALL, WM., "Introduction to Social Psychology," 7. edit. of 1912.

MORGAN, C. L., "Instinct and Experience," 1912.

SHAND, A. F., "Foundations of Character," 1914.

THORNDIKE, E. L., "Original Nature of Man" (Volume One of Educational Psychology), 1913.

TROTTER, W., "Instincts of the Herd in Peace and War," 1916.

7. *Affective View of Thought and Action*

MAIER, H., "Psychologie eines Emotionalen Denkens," 1908.

RIBOT, TH., "Psychology of Emotions," 1897.

RIBOT, TH., "Essai d'Imagination Créatice," 1900.

8. *Senses and Pain-Pleasure*

HOLLINGWORTH, H. L., and POFFENBERGER, A. T., "The Sense of Taste," 1917.

MARSHALL, H. R., "Pain, Pleasure and Esthetics," 1894.

BEHAN, R. J., "Pain, Its Origin, Conduction, Perception, and Diagnostic Significance," 1916.

MEYER, M., "Nervous Correlate of Pleasantness and Unpleasantness," in Psychological Review, 1908.

MOORE, H. TH., "Pain and Pleasure," 1917.

WOHLGEMUT, A., "Pleasure and Unpleasure," in British Journal of Psychology, 1919.

YOUNG, P. T., "Pleasantness and Unpleasantness in Relation to Organic Response," in American Journ. of Psychology, vol. 32.

NADEJDE, B., "Biologische Theorie der Lust and Unlust," 1908.

9. *Valuation*

ANDERSON, B. M., "Social Value," 1911.

BALDWIN, J. M., "Social and Ethical Interpretations," 1897.

BOODIN, J. E., "Value and Social Interpretation," in Am. Journal of Sociology, 1916.

COOLEY, CH. H., "The Social Process" (Parts Two and Six), 1918.

PARRIS, M., "Total Utility and Economic Judgment," 1909.

PICARD, M., "Psychological Basis of Value," in Journal of Philosophy, Psycholgy, and Scientific Methods, 1920.

URBAN, W. M., "Valuation, Its Nature and Laws," 1909.

BROGAN, A. P., "Urban's Axiological System," in Journal of Philosophy, 1921.

WATKINS, J. B., "Welfare as an Economic Quantity," 1914.

BRENTANO, L., "Entwicklung der Wertlehre—," 1908 (Sitzungsberichte der Kgl. Bayr. Akademie der Wissenschaften, 3. Abhandlung, 1908).

EHRENFELS, CHR. V., "System der Wert-Theorie," 2 vols., 1897.

FRISCHEISEN-KOEHLER, M., "Grundlegung der Wert-Theorie," in Jahrbücher der Philosophie, 1914.

GOTTL-OTTLILIENFELD, F. VON, "Der Wertgedanke, Ein Verhülltes Dogma—," 1897.

HAERING, TH., "Untersuchungen zur Psychologie der Wertung," in Archiv fuer die Gesammte Psychologie, 1913.

KRAUS, O., "Zur Theorie des Wertes, 1901.

KREIBIG, J. C., "Psychologische Grundlegung eines Systems der Wert-Theorie," 1902.

KRUEGER, F., "Begriff des Absolut Wertvollen als Grundbegriff der Moral-Philosophie," 1898.

MEINONG, A., "Psychologisch-Ethische Untersuchungen zur Wert-Theorie," 1894.

MEINONG, A., "Ueber Annahmen," 1910.

MUENSTERBERG, H., "Philosophie Der Werte," 1908.

SIMMEL, G., "Philosophie des Geldes," 1907.

10. *Valuation and Business*

SOMBART, W., "Quintessence of Capitalism" (transl. by Epstein, M.), 1915.

TAUSSIG, F. W., "Inventors and Money-Makers," 1915.

VEBLEN, TH., "Theory of Business Enterprise," 1904.

11. *Psychology of Marginism*

ROCHE-AGUSSOL, M., "La Psychologie Economique Chez les Anglo-Americains," 1918.

ROCHE-AGUSSOL, M., "Etude Bibliographique des Sources de la Psychologie Economique Chez les Anglo-Americains," 1919.

II. Epistemology and Metaphysics

1. *General*

Cassirer, E., "Das Erkenntnisproblem in der Philosophie und Wissenschaft der Neueren Zeit," 2 vols., 1906.

Stein, L., "Philosophische Stroemungen der Gegenwart," 1908.

Kant, I., "Kritik der Reinen Vernunft," 1781.

Schopenhauer, A., "The World as Will and Idea" (transl. by Haldane, R. B., and Kemp, J., 1891), 1818.

Becher, E., "Naturphilosophie," 1914.

Eisler, R., "Einführung in die Erkenntnistheorie," 1907.

Mach, E., "Erkenntnis und Irrtum," 1905.

Nelson, L., "Ueber das Sogenannte Erkenntnisproblem," 1908.

Reininger, R., "Philosophie des Erkennens," 1911.

Rickert, M., "Grenzen der Naturwissenschaftlichen Begriffsbildung, 1902.

Windelband, W., "Einführung in die Philosophie," 1914.

Bergson, H., "Time and Free Will" (transl. by Pogson, F. L., 1913), 1889.

Boutroux, E., "Contingency of the Laws of Nature," (transl. by Rothwell, F., 1916) 1874.

Enriques, F., "Problems of Science" (transl. by Mrs. K. Royce, 1914), 1906.

Varisco, B., "Great Problems" (transl. by Lodge, R. C., 1914), 1901.

Campbell, N. R., "Physics, The Elements," 1920.

Eddington, A. S., "Space, Time, and Gravitation," 1920.

Pearson, K., "Grammar of Science," editions of 1900 and 1911.

Russell, B., "Our Knowledge of the External World," 1914.

Russell, B., "Problems of Philosophy" (Home University Library edit.).

SCHILLER, F. C. S., "Humanism," 1912.
WHITEHEAD, A. N., "Inquiry Concerning the Principles of Natural Knowledge," 1919.
"Essays in Critical Realism," 1920 (by Drake, D., Lovejoy, A. O., Pratt, J. B., Rogers, A. K., Santayana, G., Sellars, R. W., and Strong, C. A.).

2. *On Nature of Social Science*

GOTTL-OTTLILIENFELD, F. VON, "Die Herrschaft des Wortes," 1901.
HEYMANS, G., "Gesetze und Elemente des Wissenschaftlichen Denkens," edit. of 1905.
JANSSEN, O., "Wesen der Gesetzesbildung," 1910.
KISTIAKOWSKI, TH., "Gesellschaft und Einzelwesen," 1899.
MUENSTERBERG, H., "Philosophie der Werte," 1908 (American version as "Eternal Values," 1909).
RICKERT, H., "Kultur- und Natur-Wissenschaft," 3. edit. of 1915.
STAMMLER, R., "Die Lehre von dem Richtigen Recht," 1902.
WINDELBAND, W., "Geschichte und Naturwissenschaft," 1894.

III. Logic

1. *General*

BAIN, A., "Logic, Deductive and Inductive," edit. of 1874.
BODE, B. H., "Outlines of Logic," 1910.
BOSANQUET, B., "Logic, A Morphology of Knowledge," 2 vols., 1888.
GIBSON, W. R. B., "The Problem of Logic," 1908.
HEGEL, G. W. F., "Logic" (transl. by Wallace, W., 1912, from "Encyclopedia" of 1817).
HIBBEN, J. G., "Logic, Deductive and Inductive," 1896.
HUSSERL, E., "Logische Untersuchungen," 1900.
JOSEPH, H. W. B., "Introduction to Logic," 1916.

KEYNES, J. M., "A Treatise on Probability," 1921.
LEWIS, C. I., "A Survey of Symbolic Logic," 1918.
LODGE, R. C., "Modern Logic," 1917.
MERCIER, CH., "A New Logic," 1912.
MILL, J. S., "A System of Logic, Ratiocinative and Inductive, 1843.
SCHILLER, F. C. S., "Formal Logic," 1912.
SIDGWICK, A., "The Use of Words in Reasoning," 1901.
SIDGWICK, A., "Elementary Logic," 1914.
SIGWART, CH., "Logik," 2. edit. of 1889-93.
VENN, J., "Principles of Empirical and Inductive Logic," 1889.
WINDELBAND, W., "Logic" (in Encyclopedia of Philosophical Sciences), 1913.

2. *Psychology of Reasoning*

BETZ, W., "Psychologie des Denkens," 1918.
DEWEY, J., "How We Think," 1910.
JASTROW, J., "Psychology of Conviction," 1918.
PILLSBURY, W. B., "Psychology of Reasoning," 1910.

IV. METHODOLOGY

Note: Much of the material on this subject will be found also in works on Epistemology and Logic, as given under II and III.

1. *Method in General*

CLIFFORD, W. K., "On the Aims and Instruments of Scientific Thought," 1872.
JEVONS, W. S., "Principles of Science," 3. edit. of 1879.
POINCARÉ, H., "Science and Hypothesis" (W. Scott Pub. Co.), 1905.
WESTAWAY, F. W., "Scientific Method," 1912.

2. *Methods of Social Science and Economics*

AMONN, A., "Objekt und Grundbegriffe der Theoretischen Nationalökonomie," 1912.

DURCKHEIM, E., "Les Règles de la Methode Sociale," 3. edit. of 1904.

KEYNES, J. N., "Scope and Method of Political Economy," 1891.

MENGER, C., "Untersuchungen über die Methode der Sozialwissenschaften und der Politischen Oekonomie—," 1883.

SIMIAND, F., "Methode Positive en Science Economique," 1912.

SMALL, A. W., "Meaning of Social Science," 1910.

SPANN, O., "Logische Aufbau der Nationalökonomie—," 1908.

STOLZMANN, R., "Grundzüge einer Philosophie der Volkswirtschaft," 1920.

SCHUMPETER, J., "Wesen und Hauptinhalt der Theoretischen Nationalökonomie," 1908.

TARDE, G., "La Logique Sociale," 1895.

WUNDT, W., "Logik," edit. of 1895, vol. III, chs. 3-4.

3. *Statistics as Method*

BOWLEY, A. L., "Elementary Manual of Statistics," 1910.

DAVIES, G. R., "Introduction to Economic Statistics," 1921.

FISHER, A., "The Mathematical Theory of Probabilities" (transl. by Miss Ch. Dickson), edit. of 1922.

FORCHER, H., "Die Statistische Methode als Selbstständige Wissenschaft," 1913.

KEYNES, J. M., "A Treatise on Probability," 1921.

KEYSER, C. J., "Mathematical Philosophy," 1922.

KING, W. I., "Elements of Statistical Method," 1911.

LEXIS, W., "Abhandlungen zur Theorie der Bevölkerungs- und Moral-Statistik," 1903.

MEITZEN, A., "Geschichte, Theorie, und Technik der Statistik" (transl. by Falkner, R. P.), 1891, published by American Academy of Political and Social Science.

MITCHELL, W. C., "The Making and Using of Index Numbers," being Part I of Bulletin No. 284 of Bureau of Labor Statistics, Oct., 1921.

RUEMELIN, G., "Reden und Aufsätze," vol. I, 1875.

SECRIST, H., "Introduction to Statistical Methods," 1917.

STERZINGER, O., "Zur Logik und Naturphilosophie der Wahrscheinlichkeitslehre," 1911.

VENN, J., "Logic of Chance," 3. edit. of 1888.

WELD, L. D., "Theory of Errors and Least Squares," 1916.

YULE, G. V., "Introduction to Theory of Statistics," 1911.

ZIZEK, F., "Statistical Averages" (transl. by Persons, W. M.), 1913.

V. *Ethics*

DE LAGUNA, TH., "Introduction to Science of Ethics," 1917.

DEWEY, J., "Human Nature and Conduct," 1922.

DEWEY, J., and TUFTS, J. H., "Ethics," 1908.

EVERETT, W. G., "Moral Values," 1918.

GREEN, TH. H., "Prolegomena to Ethics," 5. edit. of 1906.

HAYES, E. C., "Sociology as Ethics," 1921.

JODL, F., "Geschichte der Ethik in der Neueren Philosophie," edit. of 1906.

KANT, I., "Kritik der Praktischen Vernunft," 1788.

MURDOCK, J. G., "Economics as Basis of Living Ethics," 1913.

PAULSEN, F., "A System of Ethics" (transl. by Thilly, F.), 1899.

SIDGWICK, H., "Methods of Ethics," 4. edit. of 1890.

SIMMEL, G., "Einleitung in die Moralwissenschaft," 2 vols., 1893.

SMITH, J. H., "Economic Moralism," 1917.

STEPHEN, L., "The Science of Ethics," 1882.

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